A HISTORY OF SCIENCE, TECHNOLOGY AND PHILOSOPHY
In the 18th Century

VOLUME ONE

A HISTORY OF SCIENCE, TECHNOLOGY AND PHILOSOPHY

In the 18th Century

by A. Wolf

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DEDICATED

TO

MY FATHER

LEWIS WOLF

WHOSE LOVE OF GOOD BOOKS
HAS NEVER WANED THROUGH
PREOCCUPATION WITH LEDGERS

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PREFACE

THE appearance of the second instalment of my *History of Science* affords the desired opportunity of expressing my warm gratitude for the welcome accorded to the previous volume. I feel deeply grateful to Sir William Bragg, Professor F. Enriques, the late Professor L. N. G. Filon, Sir Henry Lyons, Sir Percy Nunn, the late Lord Rutherford, and others for their generous appreciation of my *History of Science, Technology, and Philosophy in the Sixteenth and teenth Centuries.* Numerous enquiries about the probable date of publication of further instalments have given me additional couragement in the belief that the work which I am doing is meeting a real need.

The present volume, dealing with the eighteenth century, is perhaps particularly opportune. At a time when large sections of the civilized world are reverting to barbarism, it is especially stimulating to study the age in which Europe was struggling, and successfully struggling, towards a state of enlightenment. What mankind has achieved once it will assuredly achieve again, and achieve, it is to be hoped, with a fuller realization of the need of eternal vigilance as the price of freedom, so essential to human progress.

The general plan of the book may be explained in a few words. The Sciences are arranged in the order of diminishing generality (or abstraction), beginning with Mathematics and ending with the Biological Sciences. Generally speaking, the less general sciences are dependent, for some of their data and methods, on the more general sciences. The scheme adopted here has consequently the advantage that, except in special cases, there is no need to refer repeatedly to the interrelations between the achievements of the various sciences. The story of the sciences is then followed by that of Technology in its main departments. And the closing chapters deal with what may be called the more specifically human studies, including Psychology, the Social Sciences, and Philosophy, in so far as they are of interest to the student of positive science, as distinguished from normative studies like Aesthetics and Ethics. Moreover, the order followed is that of the problems investigated, not the biographical order; the Index, however, will easily enable the reader to follow up the varied achievements of any thinker who worked in more than one field. No formal bibliography is given; but the volume is adequately documented and illustrated.

My indebtedness to others is necessarily great. I wish to express

Frontispiece to Diderot's *Encyclopédie* (1751)

A HISTORY OF SCIENCE, TECHNOLOGY, AND PHILOSOPHY
IN THE EIGHTEENTH CENTURY

CHAPTER I

INTRODUCTION

THE EIGHTEENTH CENTURY

THE seventeenth century bequeathed a great heritage to its successor; and the eighteenth century proved itself a worthy heir of the age of genius. The scientific, technical, and philosophic achievements of its predecessors were not only assimilated adequately, but they were considerably advanced in many directions. The eighteenth century has been variously described as the Age of Reason, the Age of Enlightenment, the Age of Criticism, the Philosophical Century. It was all these, and more. Perhaps the most adequate designation of it would be the Age of Humanism. It was the century in which the knowledge acquired was made known to far wider circles than had ever been the case previously, and was applied, moreover, in every possible direction in order to improve the conditions of human life. All the intellectual and moral forces of the age were harnessed to the chariot of human progress as they had never been harnessed to it before. The actual achievements, it is unfortunately true, were not commensurate with the efforts made by the leaders of the humanistic movement. The forces of darkness and oppression were too well entrenched to be easily dislodged. The protagonists of humanism were constantly obstructed and persecuted, and their writings were prohibited or destroyed by those in authority, but they were neither silenced nor dismayed. Louder and louder they voiced the cry of suffering humanity. Their cry echoed and re-echoed far and wide until the foundations of tyranny were shaken, and the walls of Jericho fell.

THE HERITAGE FROM THE PAST

We may begin with a summary account of what the eighteenth century inherited from the sixteenth and seventeenth centuries.

In Mathematics, great advances had been made during the two centuries, and several new branches had been established. In

greatly improved, and Mercator's map-projection was an important contribution to cartography.

The outstanding events in the Biological Sciences were Harvey's discovery of the circulation of the blood; the invention of the microscope, and its extensive use in the study of micro-organisms; the discovery of the sexuality of plants; the introduction of the clinical thermometer in medical diagnosis; and the growth of a more scientific spirit in medicine generally.

In the sphere of Technology, the seventeenth century had comparatively few inventions to its credit. Of these, the most important was the start made with the stationary steam-engine. A beginning was also made with mechanical calculators, including the slide-rule, and calculating machines.

Of the Social Sciences, Demography was the most progressive in the seventeenth century. The "political arithmetic" of Petty and others laid the foundations of the statistical study of social, economic, and other phenomena. The study of the rate of mortality in various age-groups led to the construction of life-tables to serve as a basis of life-insurance; and the regularity disclosed by these and similar investigations helped to establish a belief in the regularity of all social phenomena.

Philosophy enjoyed its golden age in the seventeenth century. Five great systems appeared, namely, the Materialism of Hobbes, the Dualism of Descartes, the Pantheism of Spinoza, the Idealism of Leibniz, and the Empiricism of Locke. They still constitute the principal types of philosophy; and most philosophical discussions centre in one or other of them.

Progress in Science, Technology, and Philosophy

Our next task is to give a brief indication of some of the advances made during the eighteenth century in science, technology, and philosophy.

In Mathematics, algebra was extended and systematized; trigonometry was generalized to form a branch of mathematical analysis; the calculus was developed and applied to problems in geometry, mechanics, and physics. A general theory of functions was established. Theories of equations and of infinite series were propounded. The calculus of variations was founded, and the doctrine of probability was developed. The principles of analytical geometry received more general formulation; and a beginning was made with descriptive geometry.

Mechanics was enriched with several new generalizations, namely,

the Principle of the Conservation of *Vis Viva* (anticipated to some extent by Huygens in the seventeenth century), D'Alembert's Principle, and the Principle of least Action. Mathematical analysis was increasingly applied to mechanical problems, and systematized. Progress was made with the study of the motion of fluids, and of solids moving through fluids, carefully devised hydrodynamical experiments being carried out for the purpose. A start was made with the kinetic theory of gases, the pressure of a gas being conceived as due to the impact of its moving particles, as conditioned by its density and temperature.

In Astronomy, a vast dynamic system was constructed on a Newtonian basis. The results achieved were incorporated in the *Mécanique Céleste* of Laplace. The problem of the motions of three mutually gravitating bodies was studied with special reference to the Sun, Earth, and Moon. Attention was paid to changes in the orbits of the planets due to their mutual gravitational influences. The figure of the Earth was investigated on hydromechanical principles. Improvements were made in the methods of mounting and equipping telescopes. The achromatic lens and the heliometer were invented. The aberration of light, and the nutation of the poles of the Earth were discovered. The mass, size, and figure of the Earth were determined, and the variations of gravity over its surface were studied. Various theories of the origin of the solar system were formulated by Kant, Buffon, and Laplace; and William Herschel investigated the system of the stars.

Physics made considerable progress in nearly all branches. In the study of Light the most important advance was in connection with photometry, the theoretical and experimental principles of which were formulated about the middle of the century by Lambert and Bouguer. In the study of Sound, progress was made in the determination of the conditions of the beats, pitch, intensity, velocity, medium, and audibility of sounds. The study of Heat resulted in a number of new discoveries in connection with thermal capacity, latent heat, the measurement of thermal expansion, and the mechanical theory of heat. The study of Electricity and Magnetism made rapid strides. The discoveries of the century, in this field, include the existence of two opposite states of electrification, the conduction of electricity by certain classes of bodies, the induction of charges upon conductors by the mere presence of charged bodies in their vicinity, and the passage of electricity through air at low pressure. The century also has to its credit the invention of improved frictional machines for the mechanical generation of electric charges, the condenser for their accumulation and storage, and the electroscope and electrometer for their detection and measurement. Increasing interest

in the quantitative aspects of electrical phenomena culminated in Coulomb's experimental proof that the forces between electric charges are subject to the law of the inverse square. Ideas as to the scale of electrical phenomena were greatly enlarged by the demonstration that lightning is an electric discharge, and that the whole atmosphere is normally in an electrified condition. It was also proved that the shock administered by certain marine animals to their enemies and prey was of an electrical nature. The quest for "animal electricity" led to the study of the minute charges arising from the contact of dissimilar metals, and so to the invention of the voltaic pile, and the discovery of the electric current. The explanation of electrical phenomena by reference to effluvia was superseded by the hypothesis of an electric fluid, or of two such fluids, which, by their unequal distribution, give rise to charges of one kind or the other in bodies. Analogous hypotheses were invoked to account for magnetic phenomena. It was established that a magnet acts upon other substances besides iron, attracting some and repelling others. The law of variation of the force of a magnetic pole with distance was established by Coulomb. In the realm of terrestrial magnetism, the variation of the compass was tabulated and its distribution was mapped in increasing detail, diurnal and annual fluctuations being established. The magnetic dip was charted, and attempts were made to compare the intensities of the Earth's magnetic field at various places on its surface.

The study of Meteorology was advanced by the international organization of systematic observation, and the collection of data by means of standardized instruments, and in accordance with a uniform procedure. Improvements were made in the design and use of barometers and thermometers; and new hygrometers, wind-gauges, etc., were invented.

Chemistry was systematized by Lavoisier. Important apparatus was invented for the collection and explosion of gases, for experiments on combustion and calcination, and for the synthesis of water. The conservation of matter (or rather of weight) in chemical changes was established. Progress was made in the study of chemical affinity and equivalents. And the nomenclature of chemistry was reformed, and standardized.

In Geology some progress was made in the study of volcanic rocks, and in the study of physical geology, in connection with which experimental methods were introduced for the first time.

Geographical exploration was carried on extensively. Individual travellers and organized expeditions explored Africa, Asia, North America (from the Atlantic to the Pacific), and the Pacific Ocean and its coasts. The dominating figure among the numerous explorers

was Captain Cook. Some progress was also made in geodesy, cartography, and physical geography.

In the Biological Sciences, improvements were made in classification and nomenclature. Advances were registered in the study of the morphology, anatomy, and physiology of both plants and animals, also in the study of embryology. Above all, Hales introduced new methods of experimentation with plants and animals. In Medicine, considerable improvements were introduced in the clinical training of students. Progress was made in the study of human physiology and in morbid anatomy. Some new medicaments were introduced, and a beginning was made with the remedial use of electricity. But most striking of all was Jenner's study of smallpox, and the introduction of vaccination.

Technology made great advances in nearly all its branches. In Agriculture, old methods and implements were improved, and new implements (threshers and chaff-cutters) were invented. The Textile industry saw the invention of Wyatt and Paul's spinning rollers, Arkwright's "water-frame," and various new looms. New methods of bleaching and dyeing fabrics were introduced by some of the leading chemists of the period. The scientific aspects of the problems of Building received attention, and progress was made in the construction of public and private buildings, and especially in the construction of roads, bridges (including iron bridges), canals, and lighthouses. The stationary steam-engine was greatly improved, and used extensively in mines and water-works; and, before the century ended, a beginning was made with locomotives, steam-carriages, and steam-boats. Even balloons and parachutes made a brave show. An important contribution to the further development of technology consisted in the improved manufacture of machine tools. Industrial chemistry saw the beginnings of the large-scale manufacture of sulphuric acid and alkali. The closing years of the century saw a new light in the form of coal-gas illumination.

The Philosophical Studies (Psychology, the Social Sciences, and Philosophy) likewise made progress during the eighteenth century. In some ways, indeed, they exercised the most potent influence on the century, in which almost everybody loved to pose as a philosopher, as a votary of world-wisdom. The study of man was regarded as the proper study of mankind, and so Psychology was the most popular study of the period. This was not perhaps altogether to its advantage, but it made some advances nevertheless. The threefold classification of mental processes (knowing, feeling, and willing) was definitely established, and a beginning was made with physiological and abnormal psychology. Psychology was also applied to education, especially to the education of the blind, and of deaf-mutes.

In the study of the formation of national character, Hume laid stress on the psychological factors, as against the climatic influences more commonly sponsored. Demography was greatly advanced by improvements in the collection and handling of statistical data. Economics was treated more systematically than ever before, especially by Adam Smith, who consolidated it in a very comprehensive manner. Philosophy was treated to a great extent in a psychological spirit, and suffered to no small extent from the crowd of popularizers; but it profited greatly from the sceptical method of Hume, and the "critical" method of Kant.

THE SPIRIT OF THE AGE

To understand the eighteenth century, something more is required than a knowledge of its achievements in science and technology. The story of its religious, social, economic, and political struggles does not come within the scope of this volume. But something must be said of the spirit of the age, which prompted these struggles, especially since it is intimately connected with the philosophy of the period. The analysis of the spirit of any age is at best a difficult and uncertain adventure. But an attempt must be made to characterize some of the more important traits in the character of the eighteenth century; and we propose to consider here briefly its secularism, its rationalism, and its naturalism, which between them helped to create a broad-minded humanism.

By secularism is meant here a lively interest in this world and in our earthly life, as distinguished from the attitude of other-worldliness and a concentration of interest on a life hereafter. By rationalism is here meant the attitude of confidence in the competence of the human understanding, confidence in private judgment, as distinguished from reliance on the dogmatic authority of others. Lastly, naturalism is used here in the sense of a belief in the "natural order" of things and events, or a faith in the intrinsic orderliness of the processes of Nature (including human nature), without any magical or supernatural interference.

The attitudes just indicated were characteristic of what is called "classicism," the mentality, that is to say, of the Athenians at their best, in the time of Aristotle. But they were unknown to the mediaeval mind, except in rare cases, and were only gradually resuscitated with the coming of the Renaissance, as the result of the stimulating contact with classical literature. Science itself was the offspring of these new attitudes. It was not their cause, but one of their effects. However, the wonderful progress which science made in the course of the seventeenth century, helped enormously to justify these

attitudes, and to encourage their adoption also in relation to other problems than those of science, technology, and philosophy. This is just what the leading spirits of the eighteenth century attempted to do, and not for themselves only, but for mankind at large. Hence their scathing criticism of all dogmatic claims to authority on the part of the Churches, and of the "divine right" of Kings and their minions. Hence their endeavours to make their age an "age of reason" in all things, to secure freedom of thought and of expression, and to resist State interference with the religious convictions and economic activities of its citizens. Hence the zeal to "enlighten" people, to induce them to fight for their legitimate interests, and to oppose any kind of exploitation and oppression.

The "Enlightenment" was really born in the seventeenth century and, moreover, in England. Well-known historical circumstances conspired to this end. The events which brought about the beheading of one King (Charles I, in 1649), and the deposing of another (James II, in 1688), inevitably undermined the people's faith in the "divine right" of Kings. The persecutions practised by each Church in turn, when in power, and the intrigues carried on by Kings who were not in sympathy with the dominant Church, convinced many members of all the Churches of the wisdom of mutual toleration. The rapid growth of international trade since the Navigation Act of 1651 was likewise conducive to a spirit of tolerance. Moreover, some of the greatest Englishmen of the period, including Milton and Locke, had eloquently preached the gospel of toleration. And the fact that some of the most important scientific discoveries of the seventeenth century were made by Englishmen, shows that the right spirit was present. From England the Enlightenment spread to France, and from there to Germany and other countries. And the whole movement was helped enormously by the mediation of Voltaire, who visited England in 1726, and became an ardent apostle of English science, English philosophy, English tolerance, and English common sense. His *Letters on the English* (1728) were publicly burned in Paris, but that did not prevent them from exercising a far-reaching influence. Indeed, Voltaire's activities in preaching toleration and fighting oppression were so persistent and effective that some people regard the eighteenth century as the Age of Voltaire.

The revolt against the "divine right" of Kings found expression in the contention that even monarchs have duties to their peoples. This idea was voiced by the elder Mirabeau, who, in his *L'Ami des Hommes* (1756), urged Louis XV to become a *roi pasteur*, not a *roi soleil*, and, in his *Théorie de l'Impôt* (1760), boldly insisted that a King's position as head of a nation was justified only so long as he proved to be worth more than he cost. Mirabeau was imprisoned for his

temerity, but he had had his say; and it is significant of the time that Frederick the Great of Prussia considered it wise to pose as "the first servant of the State." Conversely, the plea for individual freedom voiced the protest against the exploitation of the labouring classes by those who liked to reap where they did not sow. Kant gave philosophical expression to this protest when he insisted that every person ought to be treated as an end in himself, and not merely as a means. And the Utilitarians expressed it in their ideal of "the greatest happiness of the greatest number."

In protest against the authority of the Churches, the view gained ground that character and conduct are much more important than creeds. The writers of the period were constantly and persistently taunting the Churches with their zeal in persecuting people for doctrinal dissent and their condonation of immorality. The belief in the priority of conduct and character was voiced in the familiar couplet in Pope's *Essay on Man* (III):

> For modes of faith let graceless zealots fight,
> His can't be wrong whose life is in the right.

Its fullest expression is found in Lessing's *Nathan the Wise* (1779), which contains all that was best in the religious thought of the eighteenth century.

The predominance of humanism and humanitarianism in the spirit of the age naturally inclined it towards internationalism or cosmopolitanism. Voltaire preached against the selfish and harmful tendencies of a narrow patriotism. What may have been good enough for the heroes of Plutarch, he contended, was not good enough for the Age of Reason. The exercise of reason should unite all peoples into one universal brotherhood, and federate all countries into one great Fatherland of Humanity. This ideal was embraced by many of the great thinkers of the eighteenth century, including Kant, Herder, and Goethe, whom nobody regarded as unpatriotic on that account. Such humanitarianism, however, was too much in advance of its time. The nineteenth century witnessed the rapid growth of a narrow spirit of nationalism and jingoism, which has since then degenerated in some countries into the most inhuman barbarism. In comparison with the humanitarianism of the eighteenth century, the twentieth century appears to have reversed all the engines of human progress. Dr. Johnson (1709–84) evidently foresaw the possible criminal abuses of patriotism, when he described it as "the last refuge of a scoundrel."

DIFFUSION OF KNOWLEDGE

The eighteenth century witnessed an unprecedented spread of knowledge beyond the narrow circles of the learned. The rapid displacement of Latin by the vernacular was characteristic of the period. A whole army of writers made it their mission to popularize knowledge, including scientific knowledge, in order to advance the cause of enlightenment. The means used for the diffusion of knowledge consisted of encyclopaedias and periodicals, as well as ordinary books; and, towards the end of the century, special institutions were also established for the purpose. In these as in other respects, a beginning had already been made in the seventeenth century; but it was only in the eighteenth century that the whole movement gathered momentum.

The spread of knowledge did not, and could not, as yet concern the working classes, only the upper and middle classes. The reasons are obvious. The masses were still illiterate, and could not read the new literature even if it had been readily accessible. Actually, all sorts of obstacles were put in the way of the publication and circulation of the new books, etc., especially in France. Moreover, there were very few among the leaders of the Enlightenment who were in favour of the education of the working classes. Mirabeau, Adam Smith, and Rumford were among the few who advocated the free education of the poor. Others, like Rousseau with his faith in the natural goodness of the untutored "noble savage," thought that people could be quite happy without education. Still others, probably the large majority, were afraid of what the masses might do if they were suddenly emancipated from the beliefs and fears which helped to keep them in check. The spokesman of this class was none other than Voltaire, who, with all his sympathy for the poor and the oppressed, begged his atheistic friends to be discreet. "Philosophize among yourselves as much as you please. I fancy I hear dilettanti giving refined music for their own pleasure; but take care not to give this concert before the ignorant, the brutal, and the vulgar; they might break your instruments on your heads." It was this kind of apprehension that prompted Voltaire's well-known remark: "If there were no God, we should have to invent one." So nothing was done for the education of the masses; and it may well be that this neglect contributed later on to the callousness of the revolutionaries towards such eminent men of science as Lavoisier. Not until after the French Revolution (1789) was a very small beginning made with the free technical education of the working men, in Paris and London.

ENCYCLOPAEDIAS

It is generally agreed that the most influential work published in the eighteenth century was the French *Encyclopédie*, the first volume of which appeared in 1751, under the joint editorship of Diderot and D'Alembert. There were, however, other encyclopaedias before it and after it; and a brief account must be given also of some of these. The first important modern encyclopaedia was Pierre Bayle's *Dictionnaire historique et critique* (Amsterdam, 2 vols., folio, 1695, 1697; 2nd ed., 3 vols., 1702; 3rd ed., 4 vols., 1720, etc.—Eng. tr. 1709 etc.). This work did not contain much science. Nevertheless, it exercised a great influence. In Paris, people used to queue up every morning outside the Mazarin Library in order to get an opportunity of consulting Bayle's *Dictionary* in its early days. Bayle, as will be explained in the final chapter, was a philosophical sceptic, who regarded reason as an instrument of destructive criticism rather than of construction. He professed to be a believer, and there is no conclusive evidence against his sincerity. He certainly never questioned the validity of the moral conscience; and he probably based his religious faith on the same kind of intuition, not on reason. In his account of the various religious dogmas, and their alleged reasons, he was very destructive, though not openly so. People not unnaturally regarded his pious professions as merely a device against persecution, and regarded his adverse criticisms as his last word on the various subjects. In this they were probably mistaken. His method, however, as thus understood, or misunderstood, was copied by many of the rationalists of the eighteenth century; and Diderot adopted it deliberately in connection with his own *Encyclopédie*. "Respectable prejudices" were explained respectfully in the relevant articles; but the reader was referred to other articles in which the opposite views were expounded much more persuasively. Diderot also appropriated a considerable amount of the material contained in Bayle's *Dictionnaire*.

After Bayle's work the next interesting enterprise of the kind was Ephraim Chambers' *Cyclopaedia, or an Universal Dictionary of Art and Sciences* (1728, 2 vols., folio). It was not a great achievement, but it contained some science, and the scientific portions were considerably enlarged and improved in subsequent editions. The chief merit of Chambers' *Cyclopaedia* was that it led eventually to the publication of the great French *Encyclopédie*. About 1743, a French translation of the *Cyclopaedia*, prepared by an Englishman and a German, was offered to a Paris publisher. Quarrels and delays followed, and when eventually the Paris publisher had possession of the manuscript, and did not know what to do with it, he approached Diderot, who persuaded him to plan something much bigger. About 1746, Diderot

Voltaire

Bayle

Illustr. 4

Diderot

was entrusted with the planning of the new *Encyclopédie*. He was joined by D'Alembert, who regarded "the art of instructing and enlightening men" as "the noblest portion and gift within human reach." Much of Chambers', as of Bayle's, material was incorporated in the new work, which however was very much larger, and received contributions from almost everybody who was anybody in the France of the period. The contributors of articles included—to mention just a few—Voltaire, Rousseau, Buffon, Holbach, Euler, Mirabeau, Montesquieu, Quesnay, Turgot, and of course D'Alembert and Diderot, who was the most prolific contributor as well as editor. Difficulties with censors and publishers, State prohibitions and obstructions of many kinds, helped to delay and to maim the work. In 1757, after the publication of the seventh volume, D'Alembert, who was not a fighter like Diderot, withdrew, and left his co-editor in sole charge. Another ten volumes appeared in 1765; eleven volumes of plates between 1762 and 1772; and five supplementary volumes in 1776–77. With all its imperfections, the *Encyclopédie* was the greatest achievement of its kind, and the most potent influence on the Age of Enlightenment. The humanistic spirit of the whole enterprise was well expressed by Diderot in the article "Encyclopédie": "Man is the single term from which we ought to set out, and to which we ought to trace all back. . . . If you take away my own existence and the happiness of my fellows, of what concern is all the rest of nature to me?" What is of special interest to us is the large amount of space which science and technology take up in the *Encyclopédie*, and the numerous descriptions and illustrations which it contains of technical processes. The *Encyclopédie* was subsequently much enlarged, rearranged, and reissued under the title of *Encyclopédie Méthodique* (1788–1832). (See J. Morley, *Diderot and the Encyclopaedists*, 2 vols., 1878, etc.)

Of the other encyclopaedias which appeared in the eighteenth century, the most important were Zedler's *Grosses vollständiges Universal Lexicon aller Wissenschaften und Künste* (Halle, 1732–50), in 64 volumes with subsequent supplements, and the *Encyclopaedia Britannica* (Edinburgh, 1771) in 3 volumes. The former was intrinsically the most learned of all the encyclopaedias of the century; the latter soon established itself as the leading encyclopaedia in the English language, a position which it has maintained to the present day. The first important Italian encyclopaedia was the *Nuovo Dizionario Scientifico e curioso sacro-profano* (Venice, 1746–51), in 10 volumes, edited by Gianfrancesco Pivati, Secretary to the Academy of Sciences in Venice. The closing years of the century saw the first volumes of one of the most popular German encyclopaedias, namely, Brockhaus' *Conversations-Lexicon* (Leipzig, 1796–1808, 6 vols.).

There were, of course, also a number of smaller and cheaper works of the kind here considered. Of these, the following may be mentioned: *Lexicon Technicum* (1704) by John Harris, subsequently Secretary of the Royal Society; *Allgemeines Lexicon der Künste und Wissenschaften* (Leipzig, 1721) by J. T. Jablonski, Secretary of the Berlin Academy; *Bibliotheca Technologica* (London, 1738) by Benjamin Martin; *The Preceptor* by Robert Dodsley, with a preface by Dr. Samuel Johnson (London, 1748, 2 vols); and *Dizionario Universale* (1744) by the above-mentioned Pivati. Martin, in his Preface, tried to express the spirit of the times by doctoring a verse from the "Book of Proverbs" (xiv. 34), which he cites in the form: "Learning exalts a Nation, but Ignorance is the Shame of any People." He did his best to exalt his countrymen by publishing a large number of popular works on science.

PERIODICALS

The periodical literature of the seventeenth century consisted almost wholly of the publications of the learned societies. The two most important exceptions were the *Journal de Sçavans*, started in Paris in 1665, and *Nouvelles de la République des Lettres*, which Bayle inaugurated in Holland in 1684. The eighteenth century witnessed the development of the more popular type of periodical. A number of them written in English took the lead. The most famous of these were *The Tatler* (1709), *The Guardian* (1710), *The Spectator* (1711), and *The Examiner* (1712). The most distinguished names associated with these enterprises are those of Joseph Addison (1672–1719), Sir Richard Steele (1672–1729), and Dean Swift (1667–1745). The general aim of these, and practically of all other periodicals of the eighteenth century, was well described by Addison in an early number of the *Spectator*. "It was said of Socrates that he brought Philosophy down from Heaven to inhabit among men; and I shall be ambitious to have it said of me that I have brought Philosophy out of Closets and Libraries, Schools and Colleges to dwell in Clubs and Assemblies, at Tea-tables and in Coffee-Houses," (*Spectator*, Vol.1, No.10). The success of the *Spectator* prompted the publication of a similar periodical in Paris, the *Spectateur Français* (1722). The next important French periodical was first published, in Paris, in 1756, and its title (obviously inspired by the *Encyclopédie*, which was then in course of publication) was *Journal Encyclopédique*. The most important German periodical of the century was F. Nicolai's *Briefe die neueste Literatur betreffend* (1759), to which Lessing and Moses Mendelssohn contributed numerous essays, Mendelssohn especially. The Physiocrats (see Chapter XXX) published a special organ, the *Journal de l'Agriculture du Commerce et des Finances* (1765), for the
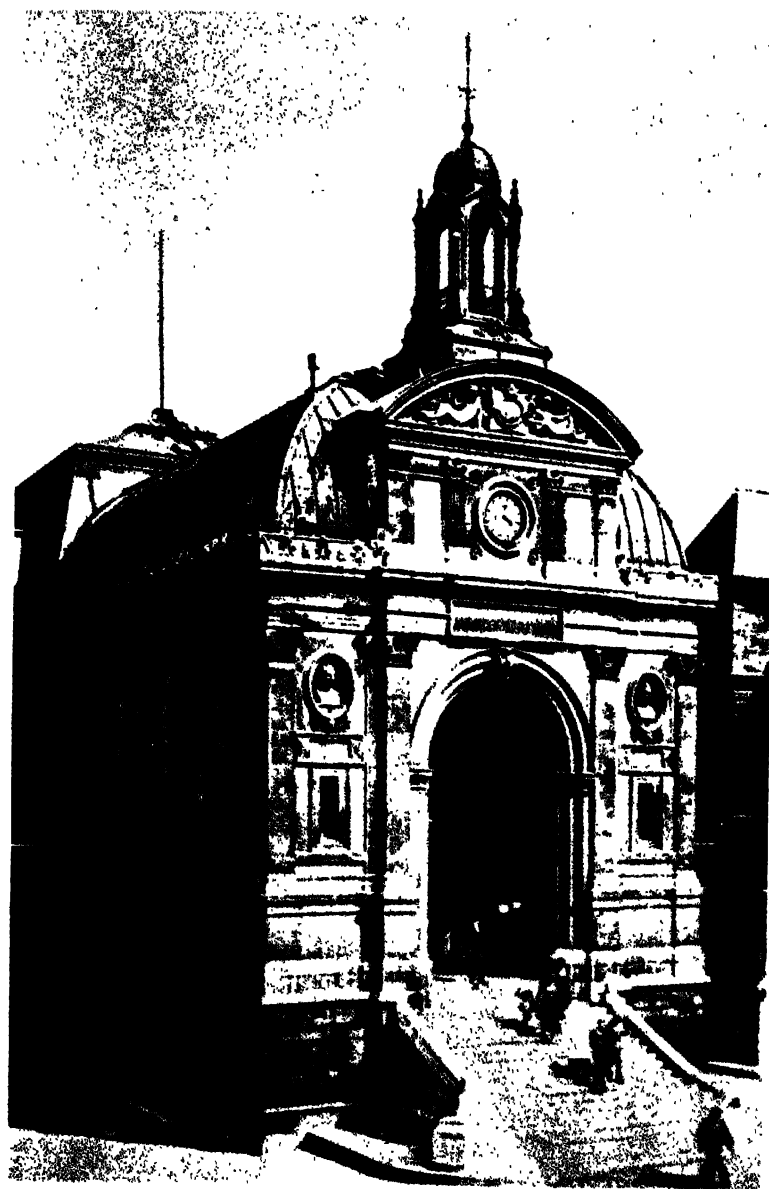
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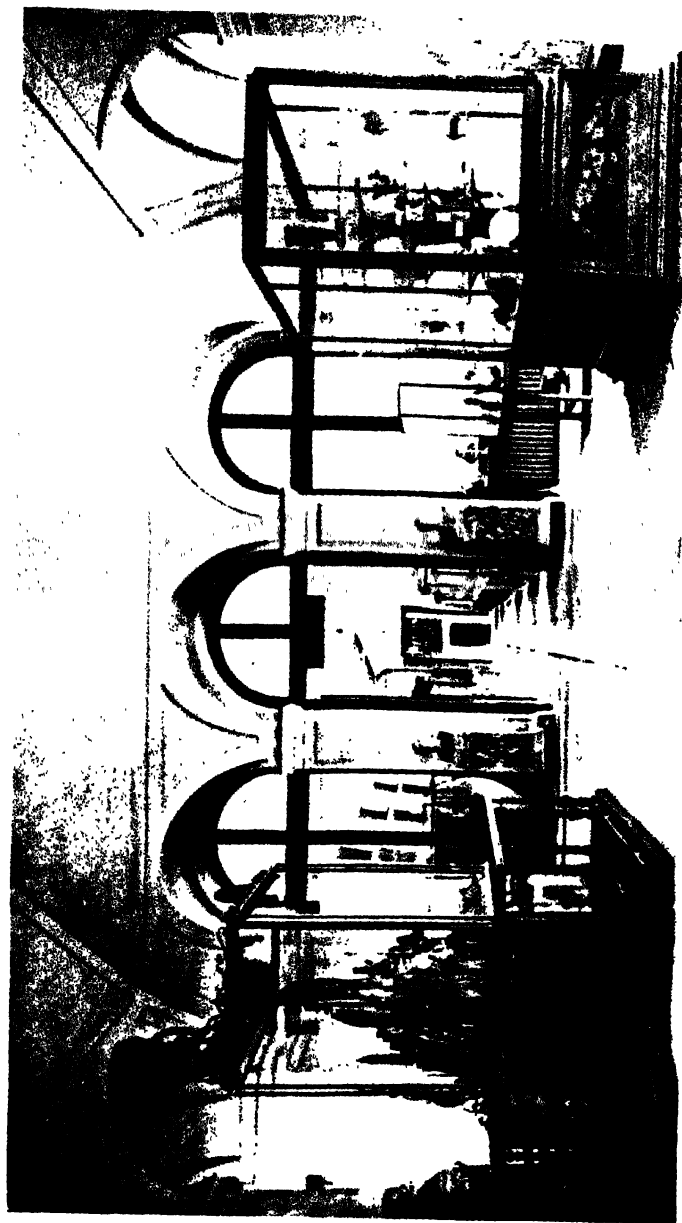
Conservatoire des Arts et Métiers—Salle de l'Écho

discussion of economic, social, and political problems. The *Journal*, which was edited by Dupont de Nemours, exercised considerable influence in advancing various economic reforms in several European countries. When the *Journal* came to a temporary end in 1766, its function was taken over by *Éphémérides du Citoyen*, dating from 1765, and originally of a more general character. It was edited at first by the Abbé Bandeau (who also belonged to the Physiocratic circle), and then by Dupont. From 1775 till 1783, the *Journal* was again the organ of the Physiocrats. Of other periodicals, reference may be made to another two English publications. In 1718 *The Freethinker* made its first appearance; it contained popular essays which were "designed to restore the Deluded Part of Mankind to the Use of Reason and Common Sense." Near the end of the century, in 1798, an altogether superior kind of periodical was published, under the editorship of Alexander Tilloch. It was called *The Philosophical Magazine*, and its "grand Object" was "to diffuse Philosophical [i.e., scientific] Knowledge among every Class of Society, and to give the Public as early an Account as possible of everything new or curious in the scientific World, both at Home and on the Continent." The *Magazine* contained many excellent papers, mostly culled from the publications of various scientific societies.

PUBLIC INSTITUTIONS

The closing years of the eighteenth century saw the establishment of two public institutions for the diffusion of scientific and technical knowledge. The two institutions referred to are still in existence. They are the *Conservatoire National des Arts et Métiers*, in Paris, and the Royal Institution of Great Britain, in London.

The *Conservatoire National des Arts et Métiers* was created by order of the Convention in 1794. In 1799, the old Benedictine priory in Saint-Martin-des-Champs, founded in the eleventh century, was taken over for the housing of the *Conservatoire*, and various collections of tools, machines, mechanical drawings, etc., were gradually accumulated. The oldest of these was a private collection of machines, etc., which had been formed by the mechanician Jacques de Vaucanson (1709–82), and bequeathed by him to Louis XVI, in 1782. This collection had been used by Vaucanson for the training of workmen. It included a silk-weaver's loom which proved an inspiration to Joseph Marie Jacquard (1752–1834), whose invention of an improved loom laid the foundations of the prosperous silk-industry of Lyons. Gradually the *Conservatoire* acquired many other collections, including Berthoud's collection of clocks, the physical apparatus of Charles and Abbé Nollet, and, perhaps most interesting

of all, the chemical apparatus originally used by Lavoisier. The last-mentioned collection is exhibited in the *Salle de l'Écho* (Illustr. 7). The original teaching staff of the *Conservatoire* consisted of three demonstrators and a draughtsman. Later on, various courses of free public lectures were instituted, and new courses were added from time to time. The activities of the *Conservatoire* have been greatly extended in recent times, and the institution has won for itself the unofficial name of *Sorbonne Industrielle*. As the first Museum of Science and Technology, the Paris *Conservatoire* may reasonably be credited with having stimulated the foundation of similar institutions elsewhere. It seems highly probable, at all events, that the decree of the Convention in 1794, to establish the *Conservatoire*, had something to do with the proposal made by Count Rumford, two years later, for the creation of something similar in London.

The Royal Institution of Great Britain owes its existence to the initiative of Sir Benjamin Thompson, Count Rumford, whose scientific work will be dealt with in later chapters. In January 1796 he proposed the establishment, in London, of an organization for providing food for the poor, and for "introducing and bringing forward into general use new inventions and improvements, particularly such as relate to the management of heat and the saving of fuel, and to various other contrivances, by which domestic comfort and economy may be promoted." His scheme further included "the formation of a grand repository of all kinds of useful mechanical inventions." Towards the end of 1796 a group of British philanthropists founded, under royal patronage, a "Society for Bettering the Condition of the Poor," and early in 1797 they made Rumford a life-member of this Society. In 1798, Rumford laid his schemes before the Society; and they appointed a committee to examine the proposals. An outline of the scheme, with a financial appeal, was drawn up and circulated by members of the committee among their friends; it invited subscriptions of fifty guineas, the subscribers and their heirs to be perpetual proprietors of the Institution. Fifty-eight influential persons immediately sent in their names; a meeting was called, and Rumford circulated "Proposals for forming by subscription, in the Metropolis of the British Empire, a Public Institution for diffusing the knowledge and facilitating the general introduction of useful mechanical inventions and improvements, and for teaching, by courses of philosophical [i.e. scientific] lectures and experiments, the application of science to the common purposes of life." Rumford defined as the two main objects of the Institution "the speedy and general diffusion of the knowledge of all new and useful improvements, in whatever quarter of the world they may
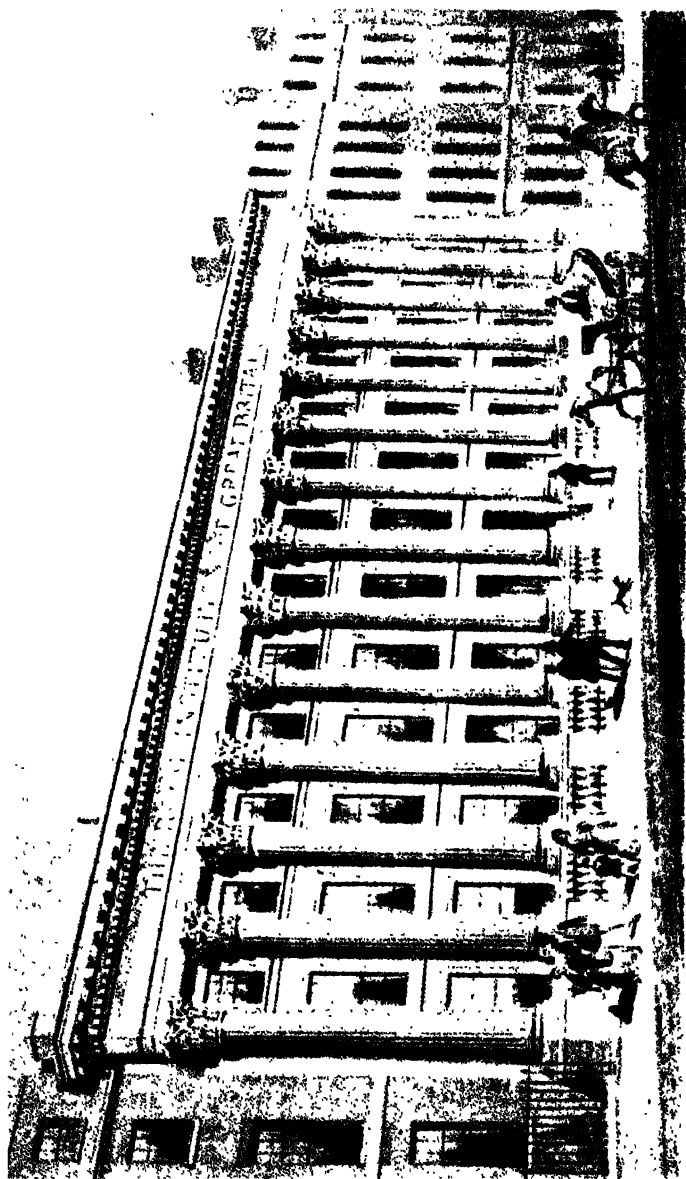
The Royal Institution of Great Britain

(From a water-colour by T. Hosmer Shepherd, 1840. By courtesy of the Royal Institution)

originate, and teaching the application of scientific discoveries to the improvement of arts and manufactures in this country, and to the increase of domestic comfort and convenience." The Institution was to contain "spacious and airy rooms . . . for the reception and public exhibition of all such new and mechanical inventions and improvements as shall be thought worthy of the public notice." In these rooms were to be housed full-sized models (*working* models, if possible) of such things as fireplaces, stoves, kilns, ventilators, kitchens and their utensils, laundries, brewing and distilling plant, spinning wheels and looms, agricultural implements, etc. Illustrated accounts of the working of these exhibits were to be provided for visitors to the repository, together with the names and addresses of the manufacturers, and the prices at which they would supply the goods. For its educational work, the Institution was to have a "Lecture-room . . . fitted up for Philosophical Lectures and Experiments; and a complete Laboratory and Philosophical Apparatus, with the necessary instruments, . . . for making Chemical and other Philosophical Experiments." Lectures were to be confined to scientific and technical topics, and the lecturers were to be men of the first rank in their several subjects. They were to give instruction on the application of the laws of heat to fuel economy and clothing, and to explain the processes involved in preserving and cooking food, making ice, tanning leather, bleaching, dyeing, and so forth. The first resident lecturer appointed by the Institution was Dr. Thomas Garnett; he acted also as Editor of the *Journal*, of which the first number appeared in 1800. Garnett's relations with Rumford, however, were unhappy, and he resigned in 1801, his place being taken by the youthful Humphry Davy. Rumford's original scheme included the establishment, as part of the Institution, of an industrial school for the technical education of artisans. Political scruples were felt in some quarters concerning the education of the lower orders, but these were overcome, and intensive courses of instruction in bricklaying, joinery, ironwork, etc., were given to carefully selected workmen. Besides the original proprietors, annual and life members were admitted to the Institution upon payment of appropriate subscriptions. Expenses were to be met by these subscriptions, by the charges for admission to the lectures and the repository, and by donations and legacies. The affairs of the Institution were to be directed by nine Managers chosen by the proprietors from their own number. Nine Visitors were also appointed to report annually on the activities and the financial position of the Institution. The Managers first met, on March 9, 1799, in the house of Sir Joseph Banks, in Soho Square; by the following June the Institution was in its permanent quarters in Albemarle Street, and soon afterwards

it received a Royal Charter from King George III. Rumford now saw most of his cherished schemes realized; but the financial expenditure was formidable. Accordingly, in a new prospectus which he drew up in 1800, he addressed himself particularly to the wealthier classes, expressing the hope that "when the rich shall take pleasure in contemplating and encouraging such mechanical improvements as are really useful, good taste, with its inseparable companion, good morals, will revive; rational economy will become fashionable; industry and ingenuity will be honoured and rewarded; and the pursuits of all the various classes of society will then tend to promote the public prosperity." When, with the dawn of the new century, Rumford left England in pursuit of other objects, the Royal Institution seemed to be drifting towards bankruptcy, and drastic retrenchment was necessary. But the achievements and popularity of Davy soon happily re-established its fortunes, and started it upon the illustrious career which it has ever since pursued.

(See also *Philosophical Magazine*, 1948, 150th Commemoration Number, "Natural Philosophy through the Eighteenth Century and Allied Topics.")

MATHEMATICS

DURING the eighteenth century pure mathematics was enriched by the development of the resources of the infinitesimal calculus; mechanics was wrought into a systematic body of theory; and the application of mathematical reasoning to experimental data greatly extended the field of mathematical physics, which had first been opened up by Galilei, Huygens, and Newton. Throughout this period mathematics and theoretical physics were more closely inter-related in their development than at any time in their previous or subsequent history. This interconnection was of benefit to both sides; the multitude of new mechanical and physical problems stimulated the purely analytical investigations which helped to solve them. The development of both pure and applied mathematics during the eighteenth century was principally the work of a few Continental mathematicians who showed equal genius in either branch. Chief among these were the Bernoullis, Euler, and Lagrange.

Somewhat less important places in the history of mathematics are occupied by the French mathematicians Clairaut, D'Alembert, Legendre and Monge. Of these, the first two did their best work in mechanics, astronomy, and mathematical physics; Legendre was primarily an analyst; and Monge founded a new branch of geometry. Laplace made fundamental contributions to mathematical physics and the theory of probability, but his greatest achievements belong to the history of astronomy. Of the British mathematicians of the period, Brook Taylor, Simpson, and Maclaurin were the out-standing figures.

A. The Calculus, Probability, etc.

THE BERNOULLIS

The Bernoullis came of a Dutch Protestant family, which had sought religious freedom in Switzerland. The oldest, and one of the most distinguished of the numerous mathematicians which the family produced, was Jakob Bernoulli (1654–1705), whose work forms a connecting link between the mathematics of the seventeenth and eighteenth centuries. He was born at Basle, where he also spent most of his life, being appointed Professor of Mathematics there in 1687. Jakob's principal contributions to mathematics lay in his systematization and advocacy of the calculus of Leibniz, and in his applications of it to differential geometry, and to physical problems. He was also a pioneer in the establishment of the calculus

of probabilities. His brother Johann Bernoulli (1667–1748), who succeeded him in the Basle professorship, extended his interests to chemistry and medicine; but he is chiefly noteworthy in the history of mathematics for his solutions, and attempted solutions, of problems involving maxima and minima, and for his establishment of analytical trigonometry. Johann's second son, Daniel Bernoulli (1700–82), became Professor of Mathematics at St. Petersburg, but later returned to hold successively several professorships at Basle. His greatest services were to mathematical physics (especially hydrodynamics) and probability. The genius of the Bernoullis extended to yet a third generation; but only Jakob, Johann, and Daniel belong to the front rank of mathematicians.

The task of generalizing the processes discovered by Leibniz so as to constitute the integral calculus on regular lines, was undertaken by the two elder Bernoullis, Jakob and Johann. It was chiefly through
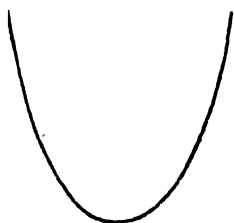
Illustr. 9.—A Catenary   Illustr. 10.—A Caustic
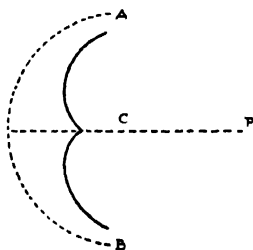
their labours that Leibniz' method of infinitesimals rapidly established itself among Continental mathematicians.

In his *Lectiones mathematicae de methodo integralium* (written in 1691–92, published in 1742, and edited in German in Ostwald's *Klassiker*, No. 194), Johann Bernoulli gave a selection from his lectures upon the methods of the integral calculus. Bernoulli begins, after some general considerations, with the quadrature of surfaces, the rectification of curves, and the solution of differential equations. He turns next to problems in mechanics and physics, such as those of caustics (first thoroughly investigated by Tschirnhaus), tautochrones, and catenaries; but the possibilities of the calculus in this direction were first thoroughly explored by Daniel Bernoulli.

(For the benefit of some readers it may be advisable to explain and illustrate the curves just named and some others which will be referred to presently. A *catenary* is the curve formed by a uniform chain hanging freely under gravity.

A *caustic* is the curve formed by the reflection of rays from a point (P) in an axial section (AB) of a concave spherical mirror.

A *cycloid* is the curve generated by a point (P) on the circumference of a circle which rolls on a fixed straight line (AB).

A *tautochrone* is a curve such that a particle starting from rest and sliding down it under the action of gravity always takes the same time to reach a certain terminal point from whatever position on the curve the motion starts. This curve was shown to be a *cycloid* with a horizontal basis. A *brachystochrone* is the curve of quickest descent, or the curve down which a particle, moving under the action of gravity, passes in the shortest possible time. This curve, too, is a *cycloid*.)

Another work by Johann Bernoulli, on the differential calculus, was long thought to be lost, but was subsequently discovered in manuscript in the Basle University Library (see Ostwald's *Klassiker*, No. 211). It now appears that this little book formed the basis of a much better known work of the period, namely, L'Hôpital's *Analyse des infiniment petits* (Paris, 1696), which, like Bernoulli's tract, deals
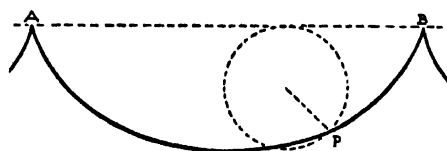
Illustr. 11.—A Cycloid

with elementary differentiation, and maxima and minima, but with some additional applications to caustics, envelopes, the theory of equations, etc.

Johann Bernoulli also did much to make trigonometry a branch of analysis. His work in this respect was supplemented by Abraham de Moivre (1667-1754), a French mathematician who settled in England. He is chiefly remembered as the discoverer of "De Moivre's Theorem" in trigonometry, and, by statisticians, for his important contributions to the theory of probability. His work on analytical trigonometry is summed up in his *Miscellanea Analytica*, London, 1730.

Jakob Bernoulli paid particular attention to infinite series. (For an annotated German translation of his *Memoirs* on this subject see Ostwald's *Klassiker*, No. 171, a collection of five tracts published at Basle in 1689-1704.) This was chiefly because such series frequently afford a means of solving problems in integration. The forerunners of the calculus had already, for this reason, concerned themselves with the development of expressions in the form of infinite series. Thus Wallis had expressed the area between a hyperbola and its asymptotes by means of such an infinite series, and already in his

writings occurs the series of reciprocals of squares of successive numbers

$$\frac{1}{1^2} + \frac{1}{2^2} + \frac{1}{3^2} + \cdot\cdot\cdot$$

whose summation, however, was first performed by Euler. One of the earliest integrations by means of a development in series was that performed by Nicolaus Mercator (? 1640–1687) in his quadrature of the rectangular hyperbola, which he used to prove his independently discovered logarithmic series (1668). Leibniz, too, had summed several infinite series leading to the evaluation
Newton enunciated his binomial theorem, for the general ca
the form of an infinite series. Jakob Bernoulli's researches in ir
series, which here interest us chiefly as leading to the development of applied mathematics, enabled him to express the relation between the co-ordinates of the elastic curve by means of such series, and to rectify the parabola and the logarithmic curve, and so forth. Euler paid particular attention to the theory of infinite series, but, like his contemporaries, he frequently used such series without ensuring that they were convergent. The establishment of a rigorous theory of infinite series began in the nineteenth century with the work of Gauss, Cauchy, and Abel.

Another branch of pure mathematics having important scientific bearings, in which Jakob Bernoulli did valuable work, was the calculus of probability. His interest in the theory of combinations and probability dated from about 1680, and he later collected the results of his own and of Huygens' researches in these departments in his great work *Ars conjectandi* (Basle, 1713; see Ostwald's *Klassiker*, Nos. 107, 108.)

Of Bernoulli's predecessors, Pascal and Fermat were the chief pioneers in establishing the mathematical theory of probability, which now finds important applications in both the physical and the biological sciences. The initial stimulus was afforded by a gambler's problem concerning the proper division of the stakes between the players in the case of an unfinished game. Pascal, in 1654, consulted Fermat on the matter, and both reached the same solution, though by different methods. From the simple original problem Pascal was led on to consider others of greater complication and generality. In the related theory of combinations, Pascal gave a correct rule for obtaining the number of possible combinations of n things taken r at a time. (See his posthumously published *Traité du triangle arithmétique*, 1665.) Pascal's method was to construct an "arithmetical triangle" two sides of which consisted of n units, while each of the other numbers was obtained successively by adding

Jakob Bernoulli

Johann Bernoulli

Illustr. 14

Euler

Illustr. 15

Lagrange

Legendre

Gaspard Monge

Maclaurin

together the number immediately above it and the number immediately to the left of it. The sum of the numbers in the rth horizontal row gives the number of possible combinations of *n* things taken *r* at a time. For example, let $n = 6$, and $r = 3$. The required "arithmetical triangle" is obtained as follows:

I	I	I	I	I	I
I	2	3	4	5	
I	3	6	10		
I	4	10			
I	5				
I					

The numbers in the 3rd horizontal row of the triangles are 1, 3, 6, 10, the sum of which is 20. Similarly with other values of *n* and *r*.

Bernoulli's book, which is divided into four sections, contains practically all the standard results in combinations in the form in which they are still expressed. By far the most important section of the work, however, is the fourth and last, in which Bernoulli sets himself the problem of applying the calculus of probability to "civil, moral, and economic conditions." In view of the entirely new paths here opened up to these branches of mathematics, it is especially to be regretted that this section remained unfinished.

Probability is defined as a degree of certainty, distinguished from absolute certainty as a part is from the whole. If absolute certainty, denoted by *a* or 1, is made up of five alternative probabilities, of which three are in favour of the occurrence of a certain event and two against it, the event possesses a degree of certainty represented by $\frac{3a}{5}$ or $\frac{3}{5}$. *A priori* and *a posteriori* probability are distinguished from each other, and the investigation culminates in Bernoulli's Theorem, which has also been called the "Law of Large Numbers." The theorem concerns the question whether, by the multiplication of observations, or by the continued accumulation of individual instances, the estimate of probability is so improved that the proportion of favourable to unfavourable cases can be finally stated in the true proportion. Bernoulli formulates this problem, and he answers it affirmatively on the strength of a mathematical demonstration. He aptly notes that the problem has, so to speak, its asymptote,

since, however much the observations may be multiplied, it is impossible to exceed a certain degree of probability that the true proportion of the cases has been found. As an example, Bernoulli considers the case of a covered urn in which, unknown to us, 3,000 white pebbles and 2,000 black ones have been placed. By repeatedly drawing out a pebble, and each time replacing it in the urn, the proportion of white to black will be ascertained to be $\frac{3}{2}$ with ever greater probability, finally bordering on certainty, with the multiplication of instances. We are thus compelled, Bernoulli maintains, to admit a certain necessity in all occurrences. For, if all events were observed throughout all eternity, probability would finally pass into full certainty. One must therefore, he holds, recognize a necessity even in things apparently most fortuitous, and must conclude that everything in the world takes place in definite conformity to law.

The theory of probability was further systematized by the labours of Laplace and Gauss in the nineteenth century, and it has come to play an important part in several branches of science, such as biometry, and the dynamical theory of gases.

The elder Bernoullis turned the attention of mathematicians once more to problems of maxima and minima, which are of such importance in physics. By their treatment of so-called isoperimetrical problems the Bernoullis laid the foundations upon which Euler, Lagrange, Legendre, and others were later able to construct the calculus of variations. (For annotated German translations of the chief contributions to the subject, down to 1837,[1] see Ostwald's *Klassiker*, Nos. 46 and 47.)

Isoperimetrical problems (in the wider use of the term) treat, in the first instance, of curves which satisfy certain maximum and minimum conditions. The oldest of these problems was that of finding which of all the curves having a given perimeter encloses the greatest area. The ancients were already acquainted with the fact that the required curve is the circle (Pappus: *Synagoge*, V, 2). The first isoperimetrical problem which Johann Bernoulli investigated concerned the *brachystochrone* or curve of quickest descent. He formulated the problem in the following words: "Two given points which are at different heights above the ground, and do not lie in the same vertical line, are to be connected by a curve upon which a movable body, starting from the upper point, shall descend,

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[1] Notably Johann Bernoulli: *Acta Erudit.*, Leipzig, June 1696, pp. 269 ff.; Jakob Bernoulli: *Acta Erudit.*, Leipzig, May, 1697, pp. 211 ff.; Euler: *Methodus Inveniendi lineas*, etc. (mentioned below); Lagrange: *Miscellanea Taurinensia*, Tom. II, 1762, pp. 173 ff., and Tom. IV, 1770, pp. 163 ff.; Legendre: *Mém. de l'Acad. Roy. des Sci.*, Paris, 1786, pp. 7 ff.

under its own weight, to the lower point in the shortest possible time." After he had himself found the solution, Johann, following the custom of the time, challenged "the most ingenious mathematicians in the whole world" to solve the problem likewise. Newton sent a correct solution of this problem to a friend on the day after it reached him. Leibniz, Jakob Bernoulli, and L'Hôpital also solved it, showing that the required curve was a cycloid. This result caused all the more surprise as Huygens had already recognized that a particle falling down a cycloidal path requires the same time to reach the lowest point of the curve whatever its starting-point. He had on this account called the curve a *tautochrone*. Thus, as Jakob Bernoulli pointed out in publishing his solution, a curve which had been investigated by so many mathematicians that it seemed there could be nothing more to discover about it, suddenly displayed an entirely new property. Jakob Bernoulli included with his solution a more complicated isoperimetrical problem, intended as a counter-challenge to Johann, which resulted in an unseemly controversy between the two brothers.

EULER

Leonhard Euler (1707–83) was a fellow-citizen of the Bernoullis. Under the tuition of Johann Bernoulli, Euler began a long career of discovery which was closely linked with that of Johann's son, Daniel, by whose recommendation he was summoned, at the age of twenty, to the St. Petersburg Academy, where he ultimately became Professor of Mathematics. He surprised the Russian mathematicians by computing in three days some astronomical tables the construction of which was expected to occupy several months. Such strenuous exertions, however, combined with the rigour of the climate, cost Euler the sight of one eye. In 1741 he was invited by Frederick the Great to the Prussian Academy of Sciences at Berlin. Here for twenty-five years he lived in the Royal Palace, and worked with unexampled activity at the reformation of mathematics. He published 121 papers, some of considerable length, in the Transactions of the Academy, the mathematical work of which he superintended after the death of Maupertuis. The total number of papers which Euler published in his lifetime has been estimated at about 700, besides 45 separate volumes. In 1766 he returned to St. Petersburg. Shortly afterwards he became totally blind, but he continued his mathematical pursuits till the very day of his death. Euler's interests and researches extended to almost every branch of mathematics, but he was at his best in analysis, which he did much to systematize, and certain branches of which he may be considered to have founded.

In continuation of the Bernoullis' investigations on isoperimetrical

problems, Euler established the calculus of variations as a separate branch of higher analysis. While Johann Bernoulli had expressed the opinion that it was hopeless to look for a general method of solving isoperimetrical problems, Euler took the first steps towards the development of a "method of finding curves which possess some property in the highest or lowest degree," in his book of that title: *Methodus inveniendi lineas curvas maximi minimive proprietate gaudentes sive solutio problematis isoperimetrici latissimo sensu accepti* (Lausannae et Genevae, 1744; see Ostwald's *Klassiker*, No. 46). The method adopted by Euler in this work, which contains many interesting worked examples, is essentially geometrical, so that the treatment of the simpler problems is very lucid. Euler explained the scope of this branch of analysis in the following words: "The calculus of variations is the method of finding the variation undergone by an expression involving any number of variables when the values of some or all of these are varied." In a supplement to his *Methodus inveniendi*, Euler explains in detail the significance of the procedure there set forth for the solution of physical problems. He believed that nothing occurred in nature that was not associated with a maximum or a minimum value of some quantity. Thus two independent methods of attacking any given physical problem were indicated, one direct and the other indirect, which tended to confirm each other, and so led to a higher degree of confidence in the solution. For example, in determining the curvature of a rope suspended at both ends, the problem could be solved either directly by considering the action exerted by gravity upon the rope, or indirectly by employing the method of maxima and minima to define the form which the rope must assume in order that its centre of gravity should lie at as low a level as possible. Both methods lead to the same curve—a catenary.

Besides helping to found the calculus of variations, Euler made valuable contributions to every branch of mathematics then in existence. He brought the labours of Vieta to completion by fashioning algebra into an "international mathematical shorthand" (Tropfke). In his *Introductio in analysin infinitorum* (1748) Euler carried the establishment of trigonometry as a branch of analysis a stage further, defined logarithms as exponents, and gave a comprehensive discussion of the curves defined by the general equation of the second degree. While thereby developing analytical geometry, he was able, at the same time, to liberate the higher calculus from the geometrical fetters which were cramping its development, and to form it into an autonomous branch of mathematics. His *Institutiones* (1755, 1768) summed up existing knowledge about the calculus. Euler was the first distinctly to conceive the notion of a mathematical

function, with which the earlier chapters of the *Introductio* deal. This conception has been well described as perhaps the most fundamental of all the creations of modern mathematics.

Euler defines a mathematical function of a variable quantity as "an analytical expression formed in any manner from that variable quantity and from numbers or constant quantities" (*Functio quantitatis variabilis est expressio analytica quomodocunque composita ex illa quantitate variabili et numeris seu quantitatibus constantibus.—Introductio*, I, i, 4). As examples of functions of the single variable z, he gives

$$a + 3z, \quad az - 4z^2, \quad az + b\sqrt{(a^2 - z^2)}, \quad c^z, \quad \text{etc.,}$$

where a, b, c stand for constants. Later he considers functions of more than one independent variable. He describes a function as *algebraic* when it is obtained by performing, on the variable and the constants which compose it, only algebraic operations, i.e., addition, subtraction, multiplication, division, raising to powers, and extraction of roots. Logarithmic or trigonometrical functions of the variable, and those which involve it as an exponent, are classed as *transcendental*. Algebraic functions are further classified as *rational* or *irrational* according as they are free from roots of the variable, or involve such roots. They are classified as *integral* or *fractional* according as the variable, on the one hand, occurs in the numerator only, or, on the other hand, occurs in the denominator, or with a negative index. Euler further distinguishes functions which are *single-valued* (*uniformes*), i.e., which assume a definite value when that of the variable is determined, and those which are *many-valued* (*multiformes*), i.e., which have several, or an infinite number of, possible values for each value of the variable.

Euler revolutionized spherical trigonometry, which is an essential instrument for astronomical calculations. In his first paper on this subject (1753, see Ostwald's *Klassiker*, No. 73), he set himself the task of deriving important theorems in spherical trigonometry from the rules of the infinitesimal calculus, arguing that it is always profitable to arrive at the same truths by various methods, for in this way new points of view may be obtained. But the new methods must necessarily be employed here, as in all other cases, if it is desired to solve a problem quite generally. The methods of attacking trigonometrical problems, in use before Euler, were applicable only to plane and spherical triangles. He recognized that if it was desired to investigate the properties of the triangles which can be formed on an arbitrary (e.g., conoidal or spheroidal) surface by joining three points of the surface by the three shortest possible lines lying wholly within it, a problem was thereby presented which could be solved only by the resources of higher mathematics. The importance of

establishing trigonometry upon such a general conception is evident when it is remembered that geodesists have to perform their measurements, not on a sphere, but, as Euler points out, on a spheroidal surface. When the triangles selected for the purpose of triangulation are very large, account has to be taken of this fact. Having derived the formulae for the sphere alone in his first paper, he went on to consider the trigonometry of higher surfaces in a later one. He pointed out that plane trigonometry could be derived from spherical, if the length of the radius of the sphere were made to approach infinity. Quite a number of the formulae of spherical trigonometry employed to-day are due to Euler. He introduced the convenient custom of denoting the sides of a triangle by the letters a, b, c, and the opposite angles by the letters A, B, C, thereby making the formulae easier to grasp, and facilitating the discovery of new relations. Euler introduced or established several familiar mathematical symbols. Thus he denoted the base of the natural logarithms by e, and used i for $\sqrt{-1}$.

LAGRANGE

Euler was succeeded as mathematical director of the Berlin Academy by Joseph Louis Lagrange (1736–1813), the greatest mathematician of his period. Lagrange was born, of French extraction, at Turin, where he became a mathematical lecturer at the Artillery School when barely 19, and where his earliest researches were published in the Transactions of a Society of his own founding. While still very young, Lagrange corresponded with Euler and D'Alembert and won a prize of the French Academy for an investigation of the libration of the Moon. He was soon recognized as the greatest living mathematician, and in 1766 he succeeded Euler at Berlin, where he worked until the death of his patron, Frederick the Great (1786), when he removed to Paris. Here he lived peacefully during the Revolution, lecturing at the *École Polytechnique*, and assisting in the establishment of the new system of weights and measures. Napoleon, who, like Frederick, was always a generous patron of science, covered him with honours. Despite ill-health and melancholy, he maintained a steady output of important memoirs covering almost every branch of pure and applied mathematics, his researches in mechanics being summed up in his masterpiece, the *Mécanique analytique*.

A new stage in the development of the calculus of variations began with Lagrange, who substituted analytical treatment for the geometrical methods employed by the Bernoullis and by Euler in this field. Lagrange brought the differential calculus and the integral

calculus into closer relationship, and investigated the effect of small variations in the limits between which expressions were integrated. His fundamental paper on the subject appeared in 1762; it was supplemented by another paper written in 1770; and further improvements were introduced in the *Mécanique analytique* of 1788. In his paper of 1762, Lagrange gave a general solution of the problem: Given a certain function Z involving variables x, y, z, and their derivatives, required to find the relation which must connect these variables in order that $\int Z$ should be a maximum or minimum. By way of illustration of his method, Lagrange considers the brachysto-chrone, which had been the starting-point of the whole series of investigations, but which he was able to treat in a more general manner than his predecessors had done.

Lagrange was also the investigator who did most to complete Euler's task of substituting analysis for the synthetic processes of earlier centuries in all branches of pure and applied mathematics. His contributions to pure mathematics dealt especially with the theory of equations (in particular, of indeterminate equations), with differential equations, with analytical geometry, and with the theory of numbers. He solved the ancient problem of finding the integral solutions of all indeterminate equations of the second degree in two variables (1768). Fermat claimed to have solved this problem, but he did not make his method known. With Euler he established the theory of partial differential equations. In 1772 appeared his investigation on the integration of such equations when they are of the first order (see Ostwald's *Klassiker*, No. 113), and seven years later he arrived at a general method of integrating linear partial differential equations in any number of variables.

LEGENDRE

Further important contributions to the calculus of variations were made by Legendre (1786), who showed how to distinguish between maxima and minima, and by Jacobi (1837).

Adrien Marie Legendre (1752–1833) forms a connecting link between the eighteenth and nineteenth centuries. He became Professor of Mathematics at the Military School and later at the Normal School of Paris, and held several Government appointments; but his career was blighted by the hostility of Laplace, who, however, occasionally made unacknowledged use of his results. Legendre excelled in some of the most technical branches of mathematics, such as theory of numbers, circular harmonics (generalized by Laplace), and elliptic functions, which he systematized. In mechanics Legendre investigated the attractions of ellipsoids on external

particles; and to him is largely due the method of least squares, which is of fundamental importance in the mathematical theory of "errors."

B. Fluxions Among British Mathematicians

During the years immediately following Newton's publication of his method of fluxions, considerable haziness prevailed among English mathematical writers as to the nature of a fluxion. There was a widespread tendency to confound fluxions with the differentials of Leibniz, and to regard them as infinitely small quantities, though this did not prevent the notion of fluxions of fluxions from being freely used. The way was thus opened for attacks on the logical basis of the method of fluxions; and full advantage was taken of this opportunity by the philosopher Bishop Berkeley.

BERKELEY

In *The Analyst: or, a Discourse addressed to an Infidel Mathematician*[1] (1734), Bishop Berkeley took especial exception to the fundamental process of deriving the fluxion of an expression with respect to a given variable by assigning an increment to the variable and subsequently making the increment vanish so as to give the required fluxion. "It should seem," he writes, "that this reasoning is not fair or conclusive. For when it is said, let the increments vanish, i.e., let the increments be nothing, or let there be no increments, the former supposition that the increments were something, or that there were increments, is destroyed, and yet a consequence of that supposition, i.e., an expression got by virtue thereof, is retained" (§ 13). "Nothing is plainer than that no just conclusion can be directly drawn from two inconsistent suppositions" (§ 15). "And what are these fluxions? The Velocities of evanescent increments. And what are these same evanescent increments? They are neither finite quantities, nor quantities infinitely small, nor yet nothing. May we not call them the ghosts of departed quantities?" (§ 35). The tract ends with sixty-seven Queries.

JURIN AND WALTON

James Jurin, who had been a student of Newton's, and who wrote as *Philalethes Cantabrigiensis*, defended the doctrine of fluxions, and appealed to the Bishop to go back to Newton's own writings, which were free from many of the logically objectionable expressions used by his followers. Another protest was addressed to Berkeley by John Walton of Dublin. Berkeley in his reply poked fun at the

[1] Probably Halley.

inconsistencies in Newton's own statements made at various times while his ideas were developing. Jurin and Walton replied, and the controversy became increasingly involved. It is not certain whether Berkeley was entirely serious. In De Morgan's opinion "*The Analyst* is a tract which could not have been written except by a person who knew how to answer it."

ROBINS

Benjamin Robins, stimulated by the *Analyst* controversy, wrote a book on fluxions, in 1735, in which the whole treatment is based upon the conception of a *limit* to which a variable can be made to approach to within any degree of nearness without ever actually reaching it. This cleared up many of the logical difficulties, but has the disadvantage that this kind of variation has but little application in science, since the limiting values (of velocity, displacement, etc.) which occur in nature are usually reached in a finite time. Robins' views were criticized by Jurin on these grounds, and another heated controversy broke out in the *Republic of Letters* (1735–37) between Jurin on one side and Robins, reinforced by Henry Pemberton, on the other, which soon crowded all other matter out of the journal and overflowed into an appendix.

TAYLOR

Such controversies served to clarify the conception of a *limit*, and gradually led to the dropping of the notion of infinitely small quantities which could properly be neglected in calculation. They also served to stimulate such progress as was possible in mathematics by the application of Newton's method of fluxions, to which, following the dispute with Leibniz, English mathematicians made it almost a point of honour to adhere. Already important contributions, both to the pure theory of fluxions and to their physical applications, had been made by Brook Taylor (1685–1731) in his *Methodus incrementorum directa et inversa* (1715). This work, which follows Newton's mature interpretation of fluxions, includes what is now called "Taylor's Theorem," though with an inadequate proof, and applies it in physics and in the theory of equations. Taylor gives a mathematical treatment of such diverse problems as the vibration of a stretched string, and the path of a ray of light through the Earth's atmosphere. He was the founder of the calculus of finite difference.

SIMPSON

Thomas Simpson (1710–61), a self-made mathematician of genius, managed to construct a theory of fluxions without recourse to infinitely small quantities, in his *New Treatise of Fluxions* (1737). He

applied fluxions with great skill to a wide range of physical and astronomical problems.

The greatest of the eighteenth-century writers on fluxions after Newton, however, was the Scottish mathematician Colin Maclaurin (1698–1746). His voluminous *Treatise of Fluxions* (Edinburgh, 1742) was the first, and for long the only, rigorous and complete survey of this branch of mathematics, and it was regarded by Lagrange as worthy to be compared with the best work of Archimedes. Maclaurin rejected the notions of infinite and of infinitesimal quantities, and sought to deduce the principles of the subject from unexceptionable axioms, so as to match the rigour of the ancients. The great skill which Maclaurin showed in his purely geometrical treatment, by means of fluxions, of physical and astronomical problems, gave the synthetic methods which he employed a new lease of life. Maclaurin also made notable advances in the pure geometry of conics and higher plane curves, being a pioneer in the investigation of pedal curves.

Following the work of Maclaurin, there was a temporary decline in rigour, evidenced by a tendency to combine Newton's inferior notation with Leibniz' inferior conceptions. It was left for the nineteenth-century mathematicians, beginning with Robert Woodhouse, to combine on the contrary the elegant notation of the Continental analysts with the valuable notion of a limit, which had been developed in Britain during the eighteenth century, and to remove the geometrical and mechanical imagery which had surrounded the theory of fluxions from the beginning. (See F. Cajori: *A History of the Conceptions of Limits and Fluxions in Great Britain from Newton to Woodhouse.* Chicago and London, 1919.)

C. DESCRIPTIVE GEOMETRY

The greatest advance of the eighteenth century in pure mathematics occurred almost entirely in the field of analysis. The close of the century, however, saw a notable development in geometry. This was the establishment of descriptive geometry, whose characteristic problem it is to give plane representations of solid figures and, from the plans so obtained, accurately to reconstruct the original solid figures.

The use of ground-plans and elevations is as old as architecture. Papyri show that the Egyptians prepared such plans for building purposes, and Vitruvius, in his book *On Architecture*, written in the time of Augustus, gives an account of the way in which the Roman

architects constructed such plans. The art thus arose directly out of the practical necessities of building, and its further development was the product, not of the study, but of the builders' workshops of the Middle Ages. The wonderful architectural feats of that period could have been achieved only by the solution of problems in descriptive geometry such as arose especially in the construction of arches. Many of the necessary constructions must, of course, have been discovered and applied empirically without any mathematical proof of their correctness. This is evident, for instance, from many sixteenth- and seventeenth-century text-books, which give constructions of importance in architecture without the slightest attempt at proof. Painters also naturally took great interest in the development of a technique for correctly representing solid forms in a plane. Hence it is not surprising that the first German book on this subject was by the great painter Albrecht Dürer, though he had several predecessors, including the Italian Franceschi, who gave a systematic account of the subject about 1480. The importance of Dürer's book, which appeared in 1525, lies not so much in the constructions which it explains as in its insistence that the perspective basis of a picture should be constructed according to mathematical precepts, and not drawn free-hand, which was the common practice, and inevitably entailed gross errors. Dürer was thus one of the prophets of the science of perspective; but the systematization of the art into a branch of mathematics, in which the results which had accumulated through the centuries were supplemented and grounded on rigorous demonstrations, was the work of the French mathematician Gaspard Monge, whose career clearly reflects the conditions of the age of the French Revolution.

MONGE

Gaspard Monge (1746–1818) was born of humble parentage at Beaune in Burgundy. His father stinted himself to give his sons a scientific education, and at the age of sixteen Monge was teaching physics at Lyons. A plan which he made of his native town won for him admission to the college of military engineering at Mézières, where, on his own initiative, he substituted geometrical methods for the tedious arithmetical processes which were being employed in constructing the plans of fortifications; and he was thus led, about 1770, to the underlying principles of his descriptive geometry. Monge rose to be a Professor at Mézières, and later went to Paris. He took a prominent part in the Revolution, being appointed to superintend the manufacture of cannon, though during the Reign of Terror he was denounced, and fled abroad. After his return to France he resumed teaching, before taking part, with Berthollet, in Napoleon's

Egyptian campaign. As Professor at the École Polytechnique, Monge flourished under the Empire, but he lost his position, and the honours with which Napoleon had covered him, upon the Restoration of the Bourbons; and he died shortly after.

For many years Monge, in deference to the wishes of the authorities at Mézières, kept quiet about his discovery of descriptive geometry. He published an account of it first in 1795, in the *Journal des Écoles Normales*, and again in 1798, in his *Géométrie descriptive*. (See Ostwald's *Klassiker*, No. 117.)

The problem with which descriptive geometry deals is, according to Monge, twofold. On the one hand, figures of three dimensions have to be reduced to figures of two dimensions, which can be shown on drawing-paper; and, on the other hand, all those relations have to be deduced, from the drawing, which arise from the shape and configuration of the solid figure so depicted. The projective method employed by Monge for the solution of this problem starts from the hypothesis that the position of a point in space is mathematically defined if its projections upon two mutually perpendicular planes are given. By the projection of a point upon a plane is here meant the foot of the perpendicular drawn from the point to the plane. Monge's projective procedure was made especially clear by his conceiving the vertical plane of projection to be rotated about its line of intersection with the horizontal plane of projection until it is brought into coincidence with the latter. Thus the vertical projection lies side by side on the same sheet with the horizontal projection; the two figures are separated by the original line of intersection of the two planes; the two projections of any given point lie in one and the same straight line perpendicular to this line of intersection; any plane is uniquely determined by the two lines in which it intersects the two planes of projection; and these two lines meet the line of intersection of the planes of projection in one and the same point. The cases considered by Monge include those of plane and curved surfaces and the more important solids, with the shapes and sizes of their intersections. Monge's descriptive geometry immediately received numerous technical applications in architecture, engineering, etc. It underwent further theoretical development during the nineteenth century through being brought into closer relation with the synthetic geometry of Poncelet and Steiner.

Monge's other contributions to mathematics chiefly concern the differential geometry of surfaces. He was an inspiring teacher, and has exercised a lasting influence on technical education in France and elsewhere.

(See F. Cajori, *A History of Mathematics*, New York, 1919; D. E. Smith, *History of Mathematics*, Boston, 1923, 1925, and *A Source Book in Mathematics*, New York, 1929; W. W. Rouse Ball, *A Short Account of the History of Mathematics*, London, 1908.)

MECHANICS

THE systematization of Mechanics in the eighteenth century was taken in hand chiefly by the Bernoullis, by D'Alembert, and by Euler, in the first instance, and the process was completed for the time being by the labours of Lagrange.

A. GENERAL PRINCIPLES

The earlier writers on mechanics had been satisfied, for the most part, with solving numerous isolated problems in all branches of applied mathematics. As each problem had to be attacked in a different way, by the employment of special artifices, only men of the highest mathematical talent could apply themselves to mechanical problems with any hope of success. During the eighteenth century, however, a number of general mechanical principles were formulated which could be applied to whole classes of problems. These were the Principle of the Conservation of Force, the Principle of Virtual Velocities, D'Alembert's Principle, the Principle of Least Action, and the dynamical equations of Euler and of Lagrange.

THE PRINCIPLE OF CONSERVATION OF FORCE

The starting-point for the reflections of Leibniz concerning the conservation of force in the universe was an assertion by Descartes, which Leibniz opposed as false. Descartes had chosen, for the measurement of force, the product of quantity of matter into velocity, which he called the quantity of motion; and he had asserted that the total quantity of motion in the universe must remain constant. Leibniz opposed this view in a memoir contributed to the *Acta Eruditorum* of 1686 (*Brevis demonstratio*, etc.). A controversy raged between the Cartesians and the Leibnizians for many years in which representatives of almost every European nation took a share. Eventually D'Alembert, in his *Traité de dynamique*, of 1743, explained that the whole controversy was merely an empty dispute about words, it being equally legitimate to measure a force by the *vis viva* which it imparts to a body upon which it acts through a certain distance, or by the momentum which it imparts to a body upon which it acts for a certain length of time. Leibniz sought to confute his opponents by taking another of Descartes' rules and expressing it in a new form with the aid of Galilei's law of falling bodies. Descartes had assumed that a force could be measured by

the product of the weight which it would raise, into the height through which it would raise it. Leibniz argued that since, by the law of falling bodies, the height to which a body rises is proportional to the square of the initial velocity, the effect of a force upon a body must be proportional to the product of the weight into the *square* of the velocity imparted, not into the simple velocity. Both sides were right except for Leibniz' error in taking the product mv^2 as the measure of the effect of the force instead of $\frac{1}{2}mv^2$.

The relation between potential and kinetic energies, and the equivalence of the forces of nature, were ideas still outside Leibniz' ken, although he shared the opinion of many of his contemporaries that heat consists in a motion of the ultimate particles of matter. He even gives a suggestive picture of the transition from molar to molecular motion when he likens the process to that of changing a gold coin for small change.

The seventeenth-century doctrine of the conservation of force had, of course, been vaguely anticipated by Epicurus and his followers in ancient times, and Voltaire maintained that Descartes had only revived an old chimera. Newton did nothing towards the introduction of the doctrine into dynamics. The notion of a closed universe containing a definite stock of force, as conceived by Leibniz, was alien to the outlook of Newton, who conceived the universe as a machine which from time to time required divine interposition from without. It followed from the laws of impact that the quantity of motion in a system of impinging bodies cannot be constant, and Newton had concluded, in contradiction to Descartes' assertion, that the total quantity of motion in the universe as a whole cannot be constant, but that two active principles were required, the first to set bodies in motion, the second to maintain the motion. Johann Bernoulli objected that, if Newton had understood the true significance of the principle of conservation, he would not have postulated two distinct principles. For the same principle through which motion is communicated effects also the preservation of that motion, not indeed as proportional to quantity of motion, but as proportional to *vis viva*, whence it follows that no motion, in this sense, can ever be lost to the universe. Leibniz, as we have seen, shared the same opinion: the total amount of force in the universe suffers no diminution, since no body ever loses force without communicating an equal amount to other bodies; and it likewise shows no increase, since no machine can ever generate force unless it receives an equivalent impulse from without, and therefore the world as a whole cannot do so.

Among the mathematical physicists of the eighteenth century, Johann and Daniel Bernoulli devoted especial attention to the

principle of the conservation of force. The most important developments of the principle during this period were those published by Daniel Bernoulli in 1750 (*Mém. de l'Acad. R. des Sc.*, Berlin, 1748, or Ostwald's *Klassiker*, No. 191). Huygens and Leibniz had considered the case of *vires vivae* which are generated by the action of a uniform gravitational field. Daniel Bernoulli, however, dispensed with the restriction to such uniform fields. He investigated the case in which the centres of attraction are in motion, as, for example, in a system of bodies which attract one another according to Newton's law of gravitation. Considering first a system of two bodies, free to approach each other, Bernoulli shows that the *vis viva* acquired by the system depends only upon the initial and final distances between the bodies. He goes on to extend his investigations to the case of three, and finally of any number of bodies; and he shows that here also the same law holds good, whatever paths the individual bodies may describe. He concludes that "Nature never belies the great law of the conservation of *vis viva*." Bernoulli thus established the principle as generally true, though with restrictions which were removed only when molecular processes came to be taken into account. He dispelled the metaphysical mists which had gathered around the principle. In order to avoid any obscurity, he preferred to formulate the principle as that of "equality between the actual fall and the potential rise," and thus connected his ideas directly with those of Huygens.

Johann Bernoulli, writing in the *Acta Eruditorum* (1735), had expressed himself as follows: "We conclude that each *vis viva* has its own definite quantity, and that whatever part of it seems to disappear really reappears in the effects proceeding from it. Hence it follows that *vis viva* is always conserved, so that the *vis viva* present in one or more bodies before they act upon one another is present in one or other of them, or in the system, after the action. That is what I call the conservation of *vires vivae*." He holds that this general law of Nature is true even where there is an apparent discrepancy. "For if the bodies are not perfectly elastic, a portion of the *vis viva* appears to be lost through compression taking place without complete restitution. But we must suppose that this compression corresponds to that of an elastic spring which is prevented from uncoiling by a stop, and thus does not give back the *vis viva* received from an impinging body, but retains it so that a loss of force does not occur." This is for Johann Bernoulli a necessity of thought, for it is universally received as an axiom, he maintains, that no effective cause can be lost, either wholly or in part, without producing an effect equivalent to that loss. Daniel Bernoulli expresses himself in similar terms in his *Hydrodynamica* of 1738. Both came near discovering the transition

from molar into molecular motion, and the equivalence of mechanical energy and heat. What were lacking at this period, and in that immediately following, were accurate numerical data establishing this equivalence. As Diderot rightly pointed out (*Pensées sur l'interpretation de la nature*, 1754, § 45, p. 61), a knowledge of the correlation of natural forces was not reached until the experimental side of physics had made further progress.

Since the principle of the conservation of *vis viva* was thus restricted to mechanics, and was not at first extended to all branches of physics, it was almost completely forgotten, so that even Kant, though he wrote on the method of estimating *vis viva*, did not mention the principle. It was by the establishment, in the nineteenth century, of the more comprehensive principle of the Conservation of Energy that the connection between the various branches of physics was first clearly apprehended, and mechanics was made the basis of them all. It is surprising that Kant, in the course of his reflections on the universe and the process of its formation, nowhere refers to the principle of the conservation of force, although, on the other hand, in his *Metaphysische Anfangsgründe der Naturwissenschaft*, he teaches that the total quantity of matter is invariable. The extension of the principle from dynamics, for which it was first seen to hold good, to all other natural processes was first effected, about the middle of the nineteenth century, by Mayer, Joule, and Helmholtz, whose relationship to Daniel Bernoulli may be compared to that in which Copernicus stood to Aristarchus of Samos.

THE PRINCIPLE OF VIRTUAL VELOCITIES

Early writers on Mechanics frequently established their propositions by implicitly utilizing the statical law now known as the Principle of Virtual Velocities, or (following Coriolis) as the Principle of Virtual Work. This principle was formulated by Johann Bernoulli in practically its modern form (although in a phraseology now obsolete) in a letter which he wrote in 1717 to Pierre Varignon, and which was published in 1725 in the latter's *Nouvelle Mécanique ou Statique* (Tome II, p. 174). Bernoulli wrote: "In all equilibrium of forces whatsoever, in whatever manner they may be applied, and in whatever directions they may act upon one another, whether directly or indirectly, the sum of the positive energies will be equal to the sum of the negative energies taken positively." By "energy" Bernoulli means the product of a force into the distance through which it moves its point of application along its line of action, or what we should call the *work* done by the force. Thus, considering a particle or an extended body which is kept in equilibrium under an arbitrary set of forces, and supposing the system to suffer a small

displacement (whether a translation or a rotation), then Bernoulli takes the product of each force into the displacement of its point of application along its line of action. He calls this displacement a *virtual velocity*, and he calls the product the *energy*, reckoning it as positive or negative according as the point of application moves in the same sense as the force, or in the opposite sense; and he asserts that, for small hypothetical displacements of the system from equilibrium, these positive and negative energies jointly amount to zero.

D'ALEMBERT'S PRINCIPLE

Jean-le-Rond D'Alembert (1717–83) derived his Christian name from the Paris church of S. Jean-le-Rond on the steps of which he was found abandoned as an infant; and his surname of Alembert, from his working-class foster-parents. His real parents appear to have been people of high rank, and his father provided for his education. From an early age he displayed marked gifts in mathematics and philosophy, and he became a member of the Paris and Berlin Academies. But he declined tempting invitations from Frederick the Great and from the Empress Catherine II, remaining in France until his death.

D'Alembert was only twenty-six years old when his *Traité de la dynamique* (Paris, 1743) was published. (For an annotated German translation, see Ostwald's *Klassiker*, No. 106.) The book is a landmark in the development of mechanics, embodying as it does a principle as simple and as fundamental for the motion of bodies as is the principle of virtual velocities for their equilibrium. The derivation of D'Alembert's Principle may be traced back to the problem of the compound pendulum. Obviously such a pendulum is merely a lever in motion, as was pointed out by the Bernoullis; and the forces acting upon each of its particles may be classified into *external* or *impressed forces*, and *internal reactions* between the particles. D'Alembert assumed that, for the whole body, the internal reactions cancel one another, and therefore contribute nothing to the motion, while the other set of forces do in fact communicate motion to the system, so that the *effective forces* are statically equivalent to the *external* or *impressed forces*. D'Alembert treated as a typical case for the application of his principle a beam fastened at one end and variously loaded at the other, forming a system which could similarly be regarded as a compound pendulum or a moving lever. D'Alembert formulated his Principle somewhat as follows: If, to each of a connected system of particles or bodies, motions are imparted which are modified by reason of the mutual connections of the particles or bodies, the resultant motion of each may be found in

the following manner. Resolve the motions imparted to the respective particles into pairs of other motions, a, a; b, β; c, γ . . . such that, if only the motions a, b, c . . . had been imparted to the bodies, they would have moved without mutual interference, while if only the motions a, β, γ . . . had been impressed upon the system, it would have remained at rest. Then a, b, c . . . will be the motions of the respective bodies in view of their mutual reactions.

In the latter part of his book D'Alembert makes numerous applications of his Principle; and he also succeeded in relating to it the theory of the motion of fluids, in his *Traité de l'équilibre et du mouvement des fluides* (Paris, 1744). He subscribed to the opinion, current in his time, that the principles of mechanics are capable of being proved; but the alleged proofs which he adduces merely amount to saying that the proposition in question is true because there is no adequate ground for maintaining the contrary. A doubt as to the status of mechanical principles was, however, expressed in a prize question, proposed about that time by the Berlin Academy, "whether the laws [of mechanics] are necessarily or only empirically true." D'Alembert's Principle obviously relates the problems of dynamics to investigations on equilibrium and the practical knowledge thereby obtained; it by no means makes experience superfluous. It serves as a model for the convenient solution of problems, and, as Mach has pointed out, it promotes not so much insight into mechanical processes as the practical mastery of them.

THE PRINCIPLE OF LEAST ACTION

An important dynamical generalization first partially formulated in the eighteenth century is that known as the Principle of Least Action, or, more accurately, of Stationary Action.

From the end of the seventeenth century much attention had been bestowed upon so-called *isoperimetrical* problems involving the determination of the conditions under which some specified quantity assumes a maximum or a minimum value. For the treatment of such problems (of which examples are given elsewhere) an appropriate technique was devised, and this was applied, in the first instance, to obtain alternative solutions of certain statical problems involving maxima and minima. Daniel Bernoulli was anxious to extend the application of such methods from statics to dynamics (e.g., to the problem of motion under a central force), and he wrote to Euler in 1741, and again in the following year, commending the matter to his attention (Fuss: *Correspondance mathématique et physique de quelques célèbres géomètres du 18ᵉᵐᵉ siècle*, vol. II). Euler's replies are not extant; but early in 1743 he evidently arrived at some solution upon which Bernoulli congratulated him in a letter dated April 23rd, of that year. Euler's results were first published in the autumn of

744, in his book on the calculus of variations (*Methodus inveniendi neas curvas*, etc.; see *Additamentum II, De motu projectorum*). He onsiders simple examples of the motion of a single particle under entral forces in a non-resisting medium, and he shows that the onditions which make $\int vds$ a minimum for the motion between iven terminal points give the same differential equations for the rbit as do the ordinary rules of dynamics. Euler's method represents he application of a correct and precise form of the principle to the implest cases. Meanwhile, however, the French mathematician and hilosopher P. L. Moreau de Maupertuis (1698–1759), had assumed n analogous principle as the basis of his explanation of the law of efraction of light. Maupertuis set forth his theory in a memoir *Accord de différentes loix de la Nature*) which he communicated to the 'rench *Académie des Sciences* on April 15, 1744 (a date intermediate etween Euler's discovery and its publication), and which was nserted in the *Recueil* for that year. In this memoir Maupertuis ummarized the various existing explanations of Snell's Law of Refraction; he thought it could best be explained on the general rinciple that the Superior Intelligence governing the Universe lways chooses the simplest means for the achievement of His ends. The opticians of the ancient world recognized that a ray of light, by ravelling in a straight line, reaches its goal in the least possible time. t was also known that the law of reflection of light involves the ame principle, since a ray travelling from one given point to another nd suffering reflection from a given plane mirror on the way, has a ninimum distance to go when its angle of incidence upon the nirror equals its angle of reflection. In the seventeenth century the 'rench mathematician Fermat showed that Snell's Law of Refraction of a ray of light at the boundary of two different media followed mmediately from the assumption that the ray takes the path of east time in going from a given point in the first medium to a given oint in the second. But Fermat's deduction carried with it the corollary that light travelled more rapidly in rarer than in denser nedia. This was in direct contradiction to the implications of the prevailing theories of refraction, to which Maupertuis adhered. He herefore rejected Fermat's explanation. But he showed that it was still possible to regard a ray of light as following the path of least action in travelling from a point A in one medium to a point B in the other, provided that such action was measured by multiplying the distance, travelled by the ray in each medium, by the velocity of light therein. That is to say, Maupertuis assumed that $(AC.v_1 + CB.v_2)$ s a minimum, and he thence deduced that

$$\frac{\sin \alpha}{\sin \beta} = \frac{v_2}{v_1} = \text{a constant.}$$

Fermat, on the other hand, had assumed that

$$\left(\frac{AC}{v_1} + \frac{CB}{v_2}\right) \text{ was a minimum,}$$

and had deduced that

$$\frac{\sin \alpha}{\sin \beta} = \frac{-}{v_2} = \text{a constant,}$$

with the velocities thus in the inverse ratio to that obtained on Maupertuis' assumption. Two years later, in 1746, Maupertuis presented to the *Académie Royale des Sciences* of Berlin (of which he was then the President) a memoir entitled *Recherche des Loix du Mouvement*. He here enunciates his *Principe de la moindre quantité d'action* in the following terms: "Whenever any change occurs in

Nature, the quantity of action employed for this change is always the least possible," the action involved in the motion of a body being taken as jointly proportional to the mass, the speed, and the distance travelled. The Principle was thus elevated to the status of a universal Law of Nature, from which, Maupertuis claimed, all the other rules of Mechanics could be derived. But the further considerations here put forward in proof (or rather in illustration) of the Principle amounted merely to deducing from it the known laws of impact of elastic and of inelastic bodies. In fact, at Maupertuis' hands, Euler's principle lost in rigour what it gained in generality. It was soon pointed out by the Chevalier D'Arcy that the "action" minimized by Maupertuis in the several applications of his Principle was not the same quantity in every case, and that it was possible to cite natural processes in which the action involved is a *maximum* (*Mém. de l'Acad.*, Paris, 1749 and 1752). Another attack on Maupertuis, by Samuel König, arose out of a claim on behalf of Leibniz for priority in the discovery of the Principle; this led to a bitter controversy in which Voltaire was involved. In the course of his early researches on the calculus of variations, Lagrange greatly extended the mechanical applications of the Principle of Least Action, and freed it from teleological associations (*Misc. Taur.*, II, 1760-1). As defined at the close of the eighteenth century, the *action* involved in the motion of a particle of mass *m* from one given point to another on its path is the space-integral of its momentum, $\int mv\,ds$, which is equivalent to

Illustr. 20.—The Principle of Least Time

the time-integral of the *vis viva*, or $\int mv^2 dt$. More generally, the action of a dynamical system in passing from one given configuration to another is defined as the sum of the actions of its particles, $\Sigma \int mv ds$, or $\Sigma \int mv^2 dt$. The Principle of Stationary Action states that the free motion of a conservative system between any two given configurations through which it passes is characterized by a stationary value of the action with respect to small hypothetical variations in the process by which it passes from the first configuration to the second. As conceived by Lagrange, the Principle was still beset by certain obscurities, and he made little use of it; but in the nineteenth century it was clarified, and underwent important developments, at the hands of Hamilton and Jacobi. Action has come to play a fundamentally important part in twentieth-century Physics; it is an absolute quantity, independent of the way in which the space-time continuum is analysed by any particular observer; and the discovery of its atomicity is the basis of the Quantum Theory.

(See A. Mayer: *Geschichte des Princips der kleinsten Action*, Leipzig, 1877.)

EULER'S EQUATIONS

Euler introduced into Dynamics the important general equations which still bear his name and which relate to the motion of a rigid body about a fixed point or about its mass-centre (*Mém. de l'Acad. R. des Sc.*, Berlin, 1758, XIV, p. 165). These led to the discovery, and partial explanation, of the variation of latitude due to the motion of the Earth's axis of rotation about its axis of figure (*ibid.*, pp. 194 ff.). Euler also laid down the fundamental equations of fluid motion (*id.*, Vol. XI).

LAGRANGE'S EQUATIONS

It was reserved for Lagrange to mould theoretical mechanics into a system, and, by combining the principle of virtual velocities with D'Alembert's Principle, to derive fundamental mechanical equations which describe the motions of any system of bodies. These important results were set forth in Lagrange's masterpiece, the *Mécanique analytique* (Paris, 1788), which laid the foundations of modern mechanics, and which occupies a place in the history of the subject second only to that of Newton's *Principia*. The two works differ in one essential respect, namely, whereas Newton derives his results purely geometrically, or synthetically, with the aid of figures, Lagrange, dispensing with diagrams, treats the subject in an entirely analytical manner. He followed the example of Euler in this analytical treatment and in his efforts to find the most comprehensive formulae

which should enable as many particular cases as possible to be treated on the same lines. In this sense Lagrange's work has been described by Mach as one of the greatest contributions to the economy of thought.

In statics Lagrange derived the general formula for the equilibrium of a given system of forces from the principle of virtual displacements. Supposing the forces $P_1, P_2, P_3 \ldots$ to act upon a connected system of particles, and the corresponding virtual displacements in the directions of the respective forces to be $p_1, p_2, p_3 \ldots$ then 'the system is in equilibrium if $P_1 p_1 + P_2 p_2 + P_3 p_3 + \ldots = 0$, or, more shortly, $\Sigma Pp = 0$, which is the fundamental equation of statics. If the particles are referred to a system of rectangular axes of co-ordinates, and each force and displacement is resolved into components parallel to these axes, the equation runs:

$$\Sigma (Xdx + Ydy + Zdz) = 0,$$

where X, Y and Z are the three components of the force acting upon a typical particle, and dx, dy, dz, the three components of the virtual displacement of that particle.

The derivation of the corresponding formulae of dynamics from the principle of virtual displacements, in combination with D'Alembert's Principle, is effected somewhat as follows: Consider a system of particles whose masses are $m_1, m_2, m_3 \ldots$ and whose co-ordinates are $x_1, y_1, z_1; x_2, y_2, z_2$, etc. Let the components of the forces acting upon the several particles be $X_1, Y_1, Z_1; X_2, Y_2, Z_2$, etc. The effective forces, measured by the mass-accelerations of each particle, are given by

$$m_1 \frac{d^2 x_1}{dt^2}, \; m_1 \frac{d^2 y_1}{dt^2}, \; m_1 \frac{d^2 z_1}{dt^2}.$$

and similarly for the other particles. These effective forces being statically equivalent to the impressed forces, by D'Alembert's Principle, we have, by the principle of virtual displacements

$$\Sigma m \left(\frac{d^2 x}{dt^2} \delta x + \frac{d^2 y}{dt^2} \delta y + \frac{d^2 z}{dt^2} \delta z \right) = \Sigma (X \delta x + Y \delta y + Z \delta z),$$

or $\Sigma \left\{ \left(X - m \frac{d^2 x}{dt^2} \right) \delta x + \left(Y - m \frac{d^2 y}{dt^2} \right) \delta y + \left(Z - m \frac{d^2 z}{dt^2} \right) \delta z \right\} = 0.$

Lagrange proceeded to derive even more general dynamical equations, which connect the kinetic energy and the potential energy of a system with the "generalized co-ordinates" defining the configuration of the system and their derivatives.

The fundamental formulae of analytical mechanics do not give

us any new information whatever about the nature of mechanical processes; they are merely based upon principles already familiar. But they provide the means of treating analytically, by standard methods, numerous particular cases which would otherwise have to be considered individually. The completion of Lagrange's work in this field awaited the further development of the calculus, and was achieved, in the nineteenth century, by the labours of such men as Gauss, Poisson, Green, Hamilton, and Helmholtz.

Lagrange's contributions to mathematical astronomy are considered in Chapter IV.

B. Special Problems

DANIEL BERNOULLI

Daniel Bernoulli applied himself especially to solving, with the aid of the new analysis, difficult mechanical problems of which the geometrical methods adhered to by Huygens, and by Newton in his *Principia*, offered no prospect of a successful solution. He must therefore be regarded as one of the principal founders of that branch of science known as *Mathematical Physics*. He definitely introduced into mechanics the principle of the conservation of *vis viva*, of which Huygens had already shown some inkling in his researches on the compound pendulum. Bernoulli employed this principle throughout the investigations on the motion of fluids, which he published in his *Hydrodynamica* (Strasburg, 1738). Although he had thus a notion of the great importance of the principle, it was reserved for the nineteenth century to establish its universal validity, and to base the whole of natural science upon it.

In his hydromechanical treatise Bernoulli deals with such subjects as the quantitative laws governing the flow of liquids from vessels, and the reactions and impacts to which it gives rise. He also studies the flow and oscillations of fluids in tubes, vortices, the principles of hydraulics, etc. The most interesting chapter, however, is the tenth, where an attempt is made to explain the experimental laws of gases mechanically. Bernoulli conceives a gas as made up of particles moving hither and thither with great rapidity, and exerting pressure on the containing vessel in virtue of their repeated impacts. By imagining a swarm of such particles to be imprisoned in a cylinder with a movable, weighted piston, and calculating the increase in pressure which must result from depressing the piston so as to decrease the volume in a certain proportion, Bernoulli deduces Boyle's Law. He attributes the increase of pressure of a gas, resulting from rise of temperature, to an increase in the velocity of the particles, the pressure being proportional to the square of this

velocity. Daniel Bernoulli was thus the founder of the kinetic theory of gases, which awaited its fuller development, in the nineteenth century, at the hands of Joule, Krönig, Clausius, and their successors.

ROBINS

Among the mechanical problems investigated during the eighteenth century were those connected with the motions of falling bodies and of projectiles. Galilei had opened a new era in mechanics by his theory of such motions, but he had been obliged to leave an essential factor, the resistance of the air, out of account, although fully realizing its importance. Newton was the first to formulate a law describing the resistance exerted by liquids and gases upon moving bodies. He postulated that the resistance exerted by a given medium upon a given body was proportional to the square of its velocity. At Newton's instance experiments were performed which verified this assumption for average velocities. It was Johann Bernoulli who first attempted to investigate the path described by a projectile under the influence of air-resistance. He found, however, that the mathematical analysis at his command was unequal to this task, and that an approximate solution of this fundamental ballistic problem could be hoped for only from a combination of experiment and calculation. The most successful advance along these lines was made by Benjamin Robins, whose *New Principles of Gunnery* (London, 1742) was edited in German by Euler under the title *Neue Grundsätze der Artillerie* (Berlin, 1745). Robins showed that Newton's law held only for small velocities; for greater velocities the resistance was found to grow with much greater rapidity than the law allowed for. In order to be able to ascertain the velocity of a missile, at any given point on its trajectory, Robins constructed his "ballistic pendulum." A body of considerable weight was suspended so that it could swing to and fro. If a ball was fired at this pendulum its velocity of impact could be deduced, in accordance with the laws of impact, from the weights of the ball and the pendulum, and the throw of the latter. For, supposing the ball and pendulum (considered as a simple pendulum), with masses m, M, respectively, to start moving after impact with a common velocity V, then the velocity v of the ball at the moment of impact could be obtained from the equation

$$mv = (M + m) \cdot V, \quad \text{which gives } v = \frac{M + m}{m} \cdot V.$$

The effects of the resistance of gases and liquids upon moving bodies have been extensively investigated both theoretically and experimentally since the time of Bernoulli and Robins, and particularly since the rise of aviation; but owing to the complexity of

the factors involved no final solution of the problem has yet been obtained.

EULER

The investigation of the catenary curve, assumed by a uniform chain fixed at each end and sagging under its own weight, had been attempted unsuccessfully by Galilei, and later by Huygens and Leibniz. The form of the curve appears first to have been correctly defined (with a proof) by Jakob Bernoulli (in *Acta Erudit.*, 1691 and 1694, see Ostwald's *Klassiker*, No. 175). Euler applied the calculus of variations to this problem. From simple catenaries, in which elasticity plays no part, he passed on to investigate the curves assumed by an elastic strip under the action of given forces. The figures arising in this way had long been known. Among the most familiar is that shown in Illustr. 21, which is the form assumed by a strip of whale-bone, or of steel, held fast at B, and subjected at its two ends, A and C, to forces in the directions AD, CD. Such problems on elastic curves, in which the theory of maxima and minima likewise plays a part, led in their turn to problems relating to the oscillations of elastic bands. Daniel Bernoulli was the first to deal thoroughly with this type of problem. If the oscillations in question were sufficiently rapid, a musical note was produced the nature of which could be experimentally investigated; and so the results of mathematical analysis could be confirmed by the methods of physics, and a deeper insight could be gained into the nature of elastic bodies. In this field of research, too, Euler played an important part. He discriminated between particular cases of the problems involved, e.g., between the behaviour of an elastic band fixed at one end, and its behaviour when fixed at both ends. He was thus able to distinguish between the oscillations of bodies which are elastic primarily by virtue of the tension under which they are maintained (e.g., elastic strings), and the oscillations of bodies which are naturally elastic. The notes produced by such oscillations were especially investigated by Chladni, who found them in good agreement with Euler's theoretical results.

One of Euler's earliest researches in applied mathematics related to Newton's theory of the tides. In view of the importance of this

Illustr. 21.—A Curve formed by forcing an Elastic Strip of Steel

subject, the *Académie des Sciences* had had numerous tidal observations made in French harbours at the beginning of the eighteenth century. It was found that these observations could only partially be explained in the light of Newton's theory. Accordingly in 1740 the Academy offered prizes for the investigation of this problem. Among the papers for which these were awarded were contributions by Euler and Daniel Bernoulli. While basing their work on the foundations laid by Newton, they were able, with the aid of higher analysis, to take into account many circumstances which conspire in determining the tides, so that, for instance, the lagging of high tide after the time of the Moon's meridian transit could be roughly calculated.

Euler's contributions to the study of light are considered in Chapter VII.

Euler was led to investigate an important problem in applied mechanics by the invention of Segner's water-wheel, described in 1750. This prompted him to write on the theory of machines worked by the reaction of moving water (*Mém. de l'Acad. Roy. des Sciences,* Berlin, 1754, pp. 227 ff., or Ostwald's *Klassiker,* No. 182). The two works of Segner and Euler have proved of fundamental importance for the construction of turbines, and even to-day Euler's treatise is not greatly out of date. In it he solves the problem of calculating the performance of an hydraulic machine corresponding to a given fall in level and consumption of water. He further shows, by a series of examples, how to calculate the greatest possible performance of the turbine under given conditions.

CLAIRAUT AND D'ALEMBERT

Clairaut and D'Alembert rendered valuable services to mathematical physics by devising methods of solving important types of differential equations which repeatedly arise in that department, as well as by their treatment of special problems. Similar contributions of a more far-reaching character were made by Lagrange, especially by his advances in the treatment of partial differential equations, and by his introduction of the notion of the potential of a body which exerts an attraction, or a repulsion, upon bodies in its neighbourhood. This conception, first called the potential by Green later, was developed by Laplace (*Mém. de l'Acad. Roy. des Sciences,* Paris, 1787, p. 252), who showed that, in free space, the potential (V) satisfies the differential equation

$$\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} + \frac{\partial^2 V}{\partial z^2} = 0.$$

Laplace also figures in the history of mathematical physics for his theory of capillarity, and for his correction of Newton's formula for the velocity of sound.

The application of still more highly developed mathematical analysis to physical problems by Fourier and Poisson belongs to the nineteenth century.

C. Pendulum Experiments

The interrelation of advances in the realm of mathematical theory, and of progress in the technique of experimentation, is well illustrated by the investigations on the pendulum which were carried out, mainly by French physicists, in the course of the eighteenth century. The main object of these investigations was to determine what the length of a simple pendulum must be in order to beat seconds, and how this length varies according to the latitude of the place of observation; but they also served to measure the acceleration of falling bodies (g), and had an important bearing upon the problem of determining the shape of the Earth. The principal laws of the motion of pendulums had been established in the seventeenth century. Galilei had recognized that the period of vibration of any given pendulum is approximately independent of the extent of its arc of swing, and is proportional to the square root of the length of the suspending thread. He had sought to utilize this isochronous property of the pendulum for the regulation of clocks. This had been achieved by Huygens, who had also obtained the formulae giving the periods of vibration, both of simple pendulums, and of extended bodies vibrating about fixed axes under gravity. By means of Huygens' formula, the acceleration of gravity could now be found from measurements of the length and period of any simple pendulum.

During the eighteenth century considerable advances were made in the methods alike of constructing and suspending simple pendulums, of allowing for the effects of temperature, and for the influence of the surrounding air, upon their motion, and of accurately comparing their rates of vibration with the indications of reliable clocks. In all the earlier pendulum experiments, the suspending thread had been gripped at its upper end in a firmly mounted metal jaw. But this arrangement involved some uncertainty as to the exact point about which the pendulum turned, and as to how its effective length should be estimated. Hence, from about the middle of the eighteenth century, knife-edge suspensions were employed (e.g., by Bouguer, see *Mém. de l'Acad. des Sciences*, 1735, p. 526). The centre of motion could then be taken as lying in the plane upon which rested the

knife-edge which itself carried the entire weight of the pendulum. This form of suspension, however, presents its own problems, because, in actual fact, the knife-edges are cylinders, as was later recognized by Laplace and Bessel. Picard had drawn attention, in 1669, to the disturbance of the rate of a pendulum clock caused by changes of temperature. It was in order to provide automatic compensation for such irregularities that Harrison invented his gridiron pendulum (1725, see *Phil. Trans.*, 1752, p. 517), and Graham his mercurial pendulum (*Phil. Trans.*, 1726, p. 40). On the other hand, La Condamine sought to measure the thermal expansion of a metal of standard length by allowing it to oscillate as a pendulum, and observing how its period varied with known variations of temperature (*Mesure des trois premiers degrés*, etc., Paris, 1751, p. 75). Attempts were also made in the eighteenth century to allow for the influence which is exerted upon the vibrations of a pendulum by the surrounding medium whose inertia offers a resistance to the passage of the pendulum through it, while its buoyancy reduces the effective gravity of the bob. Any adequate treatment of the former factor was beyond the reach of the eighteenth-century workers, owing to the great difficulties presented by such hydrodynamical problems. Newton, however, had already studied experimentally the effect of such resistance upon the *amplitudes* of oscillation of pendulums in various media (*Principia*, Bk. II, Sect. 6), while ignoring the effect upon the *periods*. So did his successors, down to Bessel. He had also, in the second edition of the *Principia* (1713), corrected Picard's estimate of the length of the seconds pendulum at Paris for the density of the air, as he explained in the third edition (1726, Bk. III, Prop. 20). But this point was subsequently neglected until Bouguer, in his pendulum experiments in South America, corrected his results for the buoyancy of the air, whose specific gravity he determined by finding the height through which a barometer must be raised from the place of observation for the mercury to fall by one line (*Figure de la Terre*, Paris, 1749, p. 340). The correction for the variation of gravity with altitude, by reducing all pendulum observations to sea-level, was also given by Bouguer (*ibid.*, p. 357), though he was obliged by his data to take account of the attraction of the matter above sea-level in the immediate neighbourhood of his place of observation. Divergences from the approximate law of the isochronism of the pendulum become appreciable when the vibrations extend over more than a very few degrees. Hence, for the sake of accuracy, the earlier workers, such as Picard, had restricted the amplitudes of pendulums under trial; but this had the disadvantage that the motion soon ceased. In 1747, however, Daniel Bernoulli showed how to reduce the observed period of vibration to that

corresponding to an infinitely small amplitude (*Pièces de prix de l'Acad. en 1747*, pp. 1 ff). To a first approximation, the period of a pendulum vibrating with amplitude a (in circular measure) stands to its period when describing infinitely small arcs, in a proportion measured by $\left(1 + \frac{1}{4} \sin^2 \frac{a}{2} \right)$, which, if a is small, may be taken as $(1 + \frac{1}{16} a^2)$.

Measurements of the length of a simple pendulum beating seconds had been made, in the seventeenth century, by Mersenne (1644), by Riccioli (1651), and by Picard (1669). The method adopted by the last-named was to adjust the length of the thread until the pendulum just kept time with a clock beating seconds, and then to measure this length w`'h a ruler (*Mesure de la Terre*, Paris, 1671, p. 139). In 1735, however, De Mairan anticipated the later "method of coincidences," although in an inferior form (*Expériences sur la longueur du pendule à secondes à Paris, Mém. de l'Acad.*, 1735, pp. 166 ff.). His procedure was to note the instants at which the pendulum under trial, and that of a clock situated just behind it, reached the extremities of their arcs, on the same side of the vertical, simultaneously. The interval between two such conjunctions represented a whole number of pendulum oscillations, and a whole number of seconds, and the period of the pendulum was easily obtained by division. An "eye and ear" form of this method, in which visual observations of the pendulum were compared with the audible ticking of a clock, was employed by Bradley at Greenwich in the years 1743-49 (Bradley's *Miscellaneous Works*, ed. Rigaud, 1832, p. 384). Some idea of the degree of refinement attained in the technique of pendulum experiments before the close of the eighteenth century is afforded by a tract on the subject by R. G. Boscovich (*Opera pertinentia ad Opticam et Astronomiam*, Bassani, 1785, Tome V, pp. 179-269). He recommended noting the coincidences between the clock and the trial pendulums when both were passing through their vertical positions, the observer concentrating his attention on the central portions of the arcs of swing by looking through a hole in a screen. This was essentially the modern "method of coincidences"; it was more accurate than De Mairan's, because the two pendulums were observed when in the same straight line with the observer's eye, and when moving with their maximum speeds. Boscovich also established Bernoulli's reduction to infinitely small arcs, and he showed how to apply the correction in the practically important case where the successive amplitudes steadily decrease in geometrical progression, as Bouguer supposed that they did. Boscovich does not appear to have applied these methods in pendulum experiments of his own, but it was the procedure advocated by him which was adopted, in all essential

respects, some years later, by J. C. Borda, and J. D. Cassini de Thury, in some elaborate investigations which they undertook with the object of determining the length, at Paris, of the seconds pendulum.

The experiments of Borda and Cassini were carried out at the Paris Observatory in the spring and summer of 1792. Their procedure consisted essentially in comparing the rate of vibration of a pendulum of known length with that of a clock whose pendulum beat seconds, and whose error was known from observations of star-transits. This clock was fixed to a massive pier (built to support a mural quadrant), and the pendulum hung down in front of the clock from a projecting block of stone (Illustr. 22). The pendulum consisted of a platinum ball about 1·5 ins. in diameter, suspended by a fine iron wire about 12 feet long, so that it should oscillate with a half-period of about 2 secs. To the lower end of the wire there was screwed an inverted copper cup (Illustr. 23), into which the bob exactly fitted, and to which it was affixed with a little grease. Thus the bob could easily be inverted after an experiment, which could then be repeated, so as to eliminate the effects of any irregularities in the figure or density of the bob. The pendulum was suspended upon a knife-edge AB (Fig. 2) resting upon a horizontal steel surface MN (Fig. 5); this was fixed to a copper plate IKL, which was screwed to the projecting block of stone. The pendulum hung down through the slot ST. The stem CD (Fig. 2), to which the upper end of the wire was attached, was counterpoised by an upward continuation EF; this carried a movable weight GH, by adjusting which the natural period of oscillation of this suspension was made equal to that of the pendulum, whose motion was thus unaffected by the inertia of the suspension. The pendulum and clock were screened from air-currents by being completely enclosed in a case, their movements being watched through a pane of glass by an observer at a telescope. The length of the pendulum was adjusted so that it should make rather less than one oscillation while the clock made two; and it was the observer's task to note the instants at which the two pendulums passed simultaneously through the vertical position, while both were moving from right to left. In order to facilitate this observation, a white cross on a dark background was attached to the bob of the clock, and the telescope was so placed that when both pendulums were at rest, the wire of the one appeared to the observer to pass through the centre of the cross on the other. Moreover, the left-hand half of the arc described by each pendulum was hidden from the observer by a screen. When the pendulums had been set swinging, the observer noted the times at which the pendulum wire bisected the cross just as both disappeared behind the screen. He noted also the amplitude of the trial pendulum at each such coincidence, so
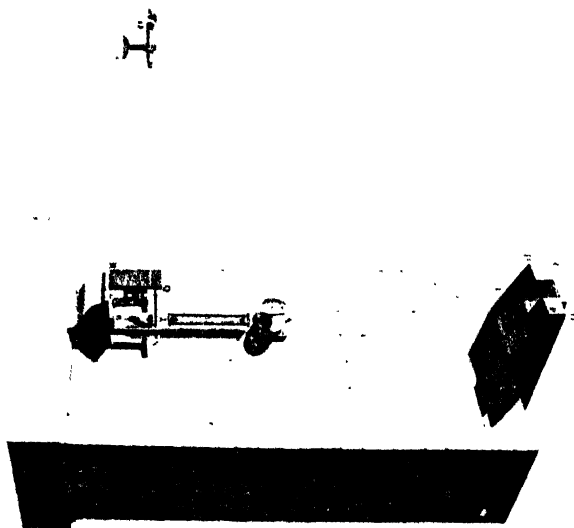
Illustr. 22

Pendulum of Borda and Cassini (1)

Illustr. 23

Pendulum of Borda and Cassini (2)
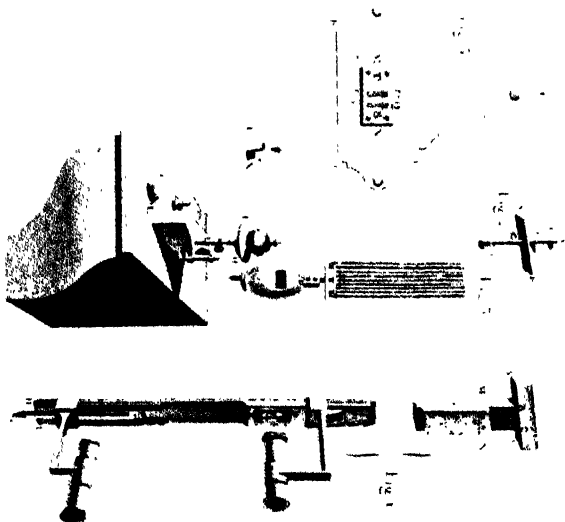
that Bernoulli's correction could be applied. By dividing the time-intervals between observed coincidences by the corrected numbers of vibrations occurring therein, Borda and Cassini were able to calculate the period of oscillation of the pendulum, and hence, taking account of the length of the suspension, to deduce the length of a pendulum which would beat seconds at Paris. The advantage of the "method of coincidences" lies in the fact that, while the uncertainty in the estimated intervals between successive coincidences represents only a small fraction of these intervals, the corresponding uncertainty in the *period of vibration* is a very much smaller fraction of this period. The length of the pendulum was measured by means of a platinum scale (Fig. 1) having, at its upper end, a steel cross-piece which rested upon the steel plate MN, so that the upper end of the scale was level with the knife-edge, while, below, it ended in a graduated tongue EF, which could be slid up and down in a groove, a vernier, X, indicating how far the end of the tongue extended below the zero of the scale. (Such an extensible scale had already been employed by Godin.) This platinum scale was covered on one side by a copper scale, which was screwed to it at one end. This combination served as a sort of metallic thermometer. For the differential thermal expansion of the two scales, measured by a special vernier, enabled the corresponding absolute expansion of the platinum scale to be calculated and applied as a correction to the measurements made with it. In making these measurements, the upper end of the scale was moved into position; a horizontal plate IH situated below the bob (Fig. 2, bottom), was then elevated by means of a screw until it just touched the lower surface of the bob, and the tongue EF was then lowered until it touched IH. The gross length of the pendulum was then given by the whole length of the scale (corrected for extension under its own weight) *plus* the prolongation of the tongue below the zero of the vernier X, the measurements being expressed to within $\frac{1}{118}$ of a line. In order to determine the length of the equivalent simple pendulum Borda and Cassini applied a number of corrections. They calculated the position of the centre of oscillation of their pendulum, and hence its effective length, from the weights and dimensions of the component parts. From the temperature and barometric pressure during the experiment they calculated the density of the air, and hence what effect its buoyancy would have in counteracting the gravity of the platinum ball. Finally, it was necessary to express the arbitrary divisions on the platinum scale in terms of those on some recognized standard; the official *toise* of the Academy was chosen. The final result of 20 sets of observations made by Borda and Cassini gave the length of the pendulum beating seconds at Paris as 440·5593 lines.

The germ of the idea of the reversible compound pendulum which, in the nineteenth century, was destined to supersede the simple pendulum for scientific purposes, is found in some recommendations, made in 1800, by G. Riche de Prony; these, however, were ignored until after the principle had been independently discovered, and put into practice, by Captain Kater in 1817.

It had been discovered, towards the close of the seventeenth century, that the seconds pendulum is shorter, and the force of gravity accordingly less intense, near the Equator than in higher latitudes. This observation had raised the question of the shape of the Earth, and had led to attempts by both Newton and Huygens to estimate the ellipticity of any section of the Earth taken through its polar axis, about which it was assumed to be symmetrical. In the course of the eighteenth century, a large number of estimates of the length of the seconds pendulum in various parts of the world were accumulated; these were mostly based upon measurements with massive pendulums of invariable length, which could be set up at one place after another, and which would go on oscillating for hours. Tables of such estimates were published from time to time, beginning with one by De Brémond (in his French translation of the *Philosophical Transactions, année* 1734, Paris, 1740, p. 126). Attempts were made to deduce a theoretical formula, which should connect the length of the seconds pendulum with the latitude of the place of observation, and should agree with such data as were available. Newton had already arrived at a simple rule, connecting the length l_ϕ in latitude ϕ with the length l_0 on the Equator, by a relation of the form

$$l_\phi = l_0 \left(1 + m \sin^2 \phi\right), \text{ where } m = \tfrac{1}{229}$$

(*Principia*, III, Prop. 20). Clairaut, working on more general assumptions than Newton as to the internal constitution of the Earth, arrived at a relation of the same form as Newton's, but assigned to m the value $\dfrac{5\,f}{2\,g_0} - \epsilon$, where

f = centrifugal acceleration at the Equator, due to the Earth's rotation;

g_0 = acceleration of gravity at the Equator;

ϵ = ellipticity of a meridian section of the Earth.

Numerous attempts have been made to deduce m, and hence the ellipticity ϵ, by comparing the results of pendulum observations the world over; but the ellipticities deduced from such comparisons do not yet agree very closely with one another, or with the geometrical ellipticities obtained from the measurement of meridian arcs.

(See *Collection de Mémoires relatifs à la Physique, publiés par la Société Française de Physique*, Paris, 1889, Tome IV, in which the memoir of Borda and Cassini is reprinted from *Base du Système Métrique*, Paris, 1810, Tome III, pp. 582 ff., and which contains also a bibliography of books and memoirs on the theory and applications of the pendulum, and a historical survey by C. Wolf.)

D. EXPERIMENTAL HYDRODYNAMICS

During the eighteenth century, a considerable amount of attention was given to problems of hydrodynamics, both on the theoretical and on the experimental sides, following upon the pioneer work of Torricelli, Mariotte, and Newton in the seventeenth century. The problems investigated were chiefly those connected with the resistance encountered by solid bodies moving through fluids, and with the efflux of liquids, under pressure, from orifices in vessels. Attempts to establish a mathematical theory of fluid resistance (even assuming the fluids to be incompressible) continued throughout the period to encounter great analytical difficulties and to yield results which were on the whole disappointing when compared with the relevant experimental data. The theories of fluid resistance which were current in the early part of the eighteenth century were generally based upon the conception of a fluid as composed of isolated particles which resisted a body in relative motion to them, merely by virtue of their impacts upon its surface. This hypothesis suggested that the resistance encountered by a plane surface, moving through a fluid in a direction perpendicular to its own plane, should be proportional to the extent of the surface and to the square of its velocity; while the oblique faces of a triangular prism, moving through the fluid like the prow of a boat, with its base perpendicular to the direction of motion, should encounter a resistance proportional to the square of the sine of the inclination of these faces to the direction of motion. It was further assumed, following Newton, that the resistance encountered by a body was determined only by the shape of the portion of the body which lay in front of the plane of its greatest cross-section (taken at right angles to the direction of motion), so that the shape of the hinder portion did not affect the value of the resistance in any way.

This excessively simple theory was criticized by D'Alembert, in his *Essai d'une nouvelle Théorie de la Résistance des Fluides* (Paris, 1752), particularly on the ground that it ignored what happened to the particles of the medium after they had impinged upon the body. He attached great importance to the part played by each layer of particles *after* impact, as it glides over the surface of the body,

exerting pressure and frictional forces upon it, and interfering with the impact of the succeeding layer; and he tried to trace out the "stream-lines" (*filets*) along which the particles of the medium move when a state of steady motion has been attained. D'Alembert sought to base hydromechanics on sound first principles connected with those which he had established for the mechanics of solid bodies. But he ran into great analytical difficulties when he tried to derive experimentally verifiable deductions from his theory. D'Alembert regarded the experimental evidence on the behaviour of fluids, which was available when he wrote his *Essai*, as too crude and discordant to be of much use to hydrodynamical theory. But about twenty years later a new era in the experimental treatment of the subject was begun by some investigations in which D'Alembert himself took part. His colleagues were the Marquis de Condorcet and the Abbé Bossut. In 1775 Bossut published a treatise on hydrodynamics in which he discussed the motion of water in pipes and canals, on the basis of some observations of his own. In the same year, D'Alembert, Condorcet, and Bossut were commissioned by Turgot to carry out researches directed towards the improvement of navigation; and they undertook an experimental investigation of the laws of the resistance of liquids to bodies passing through them. Their experiments were carried out between July and September, 1775, on a lake in the grounds of the École Militaire. Bossut acted as *rapporteur*, and the course and results of the investigation are clearly set forth in his *Nouvelles Expériences sur la Résistance des Fluides* (Paris, 1777).

The procedure adopted by the three Encyclopaedists was to measure the velocities acquired by boats towed through the water under known forces. The motive power in each case was supplied by a descending weight; this was attached to one end of a cord which was passed over a pulley-wheel at the top of a mast about 75 feet high, and under another pulley-wheel at the foot, and whose free end was then attached to the prow of the boat so as to draw it forward in a horizontal direction as the weight descended (Illustr. 24). The effect of the slight sagging of the cord under its own weight was neglected. The lake over which the boats were towed was roughly rectangular in outline, and measured about 100 feet by 50 feet, the depth varying up to about 6 feet. Its two longer sides were partly measured off into lengths of 5 feet each, the divisions corresponding to 0, 5, 10 . . . 45, 50 feet being marked by upright stakes. The lines joining corresponding divisions on opposite banks were perpendicular to the direction in which the boats moved, and the divisions marking 50 ft. lay close to the end of the lake where the boats were arrested against a mattress. Each boat was started some way behind the
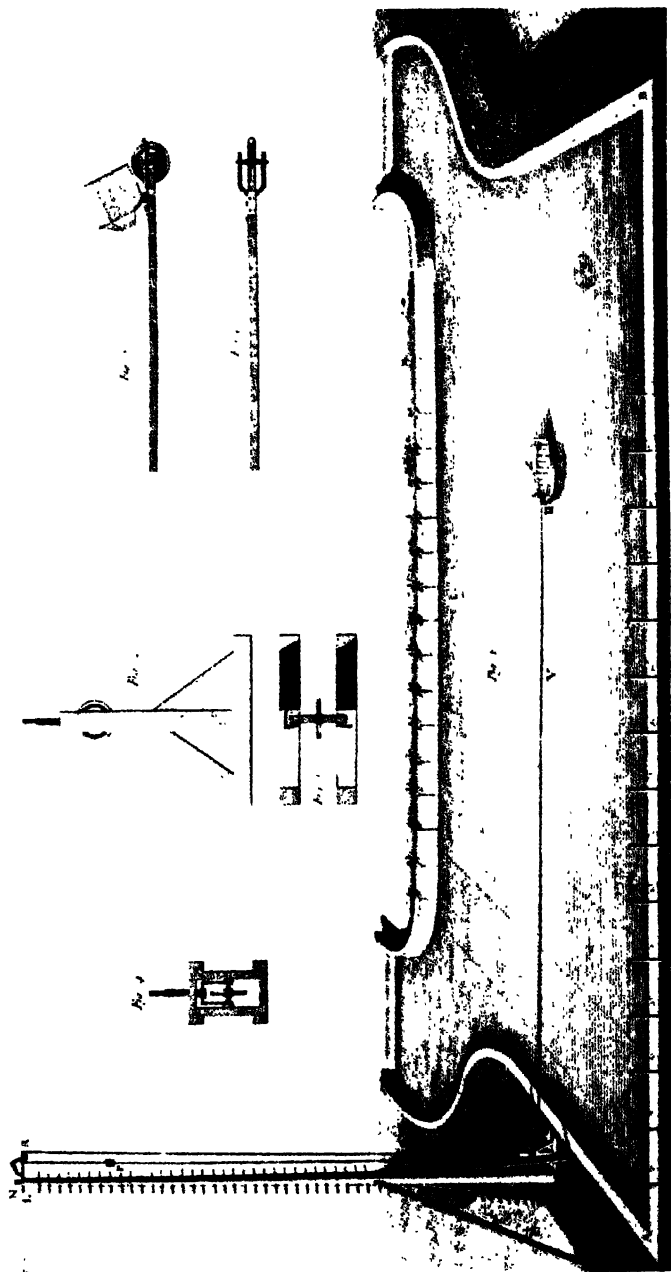
Hydrodynamic Experiments with Boats

zero line, so that it should have attained a uniform terminal velocity before entering upon the measured course of 50 ft. Its motion over that course was then timed by observers who noted the instant at which the boat crossed each of the lines (0–0, 5–5, etc.) joining the opposite pairs of posts, while a time-keeper with a watch audibly counted out the half-seconds. Besides these experiments on the motion of boats through water of practically unbounded extent in relation to the size of the vessels, there were others in which the same boats were propelled through a canal whose breadth and depth could be varied at will within certain limits. This canal was constructed by building a submarine platform along the lake, surmounted by two parallel wooden barriers which formed the sides of the canal, and which could be set at various distances apart. The depth of the canal could be varied by admitting more or less water into the lake. Most of the experiments of this type were performed with the canal open at both ends, though a few observations were also made with both ends closed. Twenty different boats were employed in these experiments, having variously shaped bows and sterns, some square, others more or less sharply pointed. The boats also differed in beam and in draught; they mostly drew about one foot of water. In order to make the boats move straight in the open water, rudders were fitted; but in the canal experiments it was necessary to use guiding-ropes passing between pairs of pulley-wheels on the bow and on the stern, a correction being applied for the friction at the pulleys. The observations were repeated a number of times under each given set of conditions, the mean of the times required by the boat to cover the 50 feet being taken; the sets of results obtained for this purpose scarcely ever differed among themselves by as much as one second.

The theoretical conclusions set forth in the *Nouvelles Expériences* were based upon hundreds of determinations of the mean velocities of the boats under suitably varied conditions. On the whole, the law that resistance varies as the square of velocity was fairly well upheld, though it was found that the resistance grew at a slightly greater rate than that required by theory. But this was largely accounted for by recognizing that the effective resistance must be increased by the formation of a swell (*remou*), or elevation of the water surface, in front of the bow, and by the formation of a depression astern; and in order that some allowance might be made for this effect, the heights of the swell at the centre and at the sides of the bow were measured as soon as the boat had reached a steady velocity, and were recorded with the other data of the experiment. The comparison of the resistances encountered by bodies whose velocities were equal but whose surfaces were unequal involved a distinction between

two cases. (1) When the surfaces immersed were of equal depths but of different breadths, the resistance was found to increase at a slightly greater rate than the surface. (2) When the surfaces had equal breadths but unequal depths, the resistance increased at a slightly lesser rate than the surface. The discrepancies in each case were explained by allowing for the effect of the swell. Comparison was next made of the results obtained with a square-bowed boat, and with boats of equal beam and draught, and sensibly equal velocities, but having bows whose sides met at various angles, so as to strike the water at various obliquities. It was found that the "sine squared" law broke down more and more seriously as the bow became sharper; the motive weights were greater than those required by theory, and the more so the sharper the bow was. Similar results had already emerged from some experiments of Borda's on oblique resistances (*Mém. de l'Acad.*, Paris, *années* 1763 and 1767). It was found that no other single power of the sine of the angle of incidence could be substituted for the square as an accurate measure of the resistance encountered. The resistance to the boats was found to be lessened by making the stern taper to a point. In the canal experiments, the proportionality between the resistance and the square of the velocity was found to hold fairly well. In each case, however, the resistance was greater than it would have been in open water, other things being equal; and this increase of resistance was the more pronounced the less the space that was left for water to pass from before the boat to its rear. An attempt was made to arrive at some sort of estimate of the absolute resistances offered by the water to the moving boats. Allowance was made for the part played by air-resistance, whose proportion to water-resistance was inferred from the relative densities of the two media, and the extents of the surfaces respectively exposed to them. The tenacity of the water, and its friction along the sides of the boats, were believed to be negligible. From the numerical results obtained, the conclusion was reached that a plane surface moving through an unbounded fluid with a velocity V in a direction perpendicular to itself encounters a resistance equal to the weight of a column of the same fluid having as its base the surface exposed to pressure, and as its height, that through which a body would have to fall to acquire the velocity V.

The publication of Bossut's treatise on hydrodynamics, in 1775, and of the results of his experiments in collaboration with D'Alembert and Condorcet, inspired the Chevalier P. L. G. Dubuat to undertake still more elaborate investigations. These were carried out in the years 1780-83, and they are described in the later editions of his *Principes d'Hydraulique* (Paris, 1779; revised edition, 1786). Dubuat's researches dealt with the laws of the uniform motion of water in

canals, the impacts of effluent jets of liquid, the distribution of pressure over the surfaces of solids of various forms moving relatively to resisting media, and with certain effects accompanying the oscillation of pendulums in such media. Many other matters of more especially technological interest are dealt with in the book. Dubuat experimented with artificial canals of wood, whose breadths, gradients, and depths of water could be varied at will. He conceived the steady flow of water down the bed of such a canal, when inclined to the horizontal, as indicating a state of equilibrium between the accelerating force of gravity upon the water and the opposing forces due to the viscosity of the water, and its friction against the sides of the canal. By varying the depth, breadth, and gradient of the stream, one at a time, and noting the concomitant variations in the velocity attained by the water, he succeeded in arriving at an empirical formula connecting this velocity with those factors. If V be the mean velocity of the stream; r the cross-section of the stream divided by the perimeter of the cross-section; and if the gradient be 1 in b, then the relation between these quantities was given by

$$V = \frac{307(\sqrt{r} - 0 \cdot 1)}{\sqrt{b} - \log_e \sqrt{b + 1 \cdot 6}} - 0 \cdot 3(\sqrt{r} - 0 \cdot 1),$$

the quantities being measured in terms of (English) inches and seconds, and the canal being assumed to be of constant section and gradient, and of considerable length in comparison with its other dimensions (*Principes d'Hydraulique*, 1816 ed., Vol. I, pp. 62 f.). Dubuat also investigated the relation between the velocities of the water at the surface and at the bottom of the stream respectively. He found that, if V is the velocity at the surface, and v that at the bottom, then $v = (\sqrt{V} - 1)^2$, and that the mean velocity over the whole cross-section of the stream (deduced from the discharge from the canal in unit time) was the arithmetic mean of V and v (*ibid.*, p.90). These results appeared to be independent of the size, shape, and gradient of the bed. He measured the surface velocities by throwing a chip of wood into midstream, and timing its motion over 10 fathoms of the canal with a watch. For measuring the velocity of the lowest layer of the stream, he similarly observed the motion along the bottom of little balls made of some substance only slightly denser than water, and of sufficiently bright hue to be easily visible— a red currant was found to serve the purpose best. Formulae of greater generality for the motion of running water in canals were obtained early in the nineteenth century by G. Riche de Prony, who, however, largely based his rules on selected experiments performed by earlier workers. Dubuat studied the impacts of effluent jets by

means of a glass tube, open at both ends, which was bent through a right angle, and was mounted with one limb horizontal and the other pointing vertically upwards. The open end of the horizontal limb was inserted into a perforation in a metal plate (plates of various shapes were employed) so as just to lie flush with the surface of the plate; it was then thrust end on into the jet of water, whose diameter was greater than that of the tube; and the experiment consisted in ascertaining the height at which the pressure of the jet would maintain a column of water in the vertical limb of the tube. Dubuat showed that this height was approximately equal to that of the head of pressure maintaining the jet. This held good for a range of values of the head of pressure and diameter of the jet, provided the impact occurred not very far from the orifice, and that the issuing water was not made to pass through a nozzle. Dubuat also investigated the distribution of pressure over the surface of a solid immersed in a stream of water. He did this by means of a metal box whose walls had perforations at various points, which could be opened or closed at will. A manometer, communicating with the interior of the box and rising above the surface of the water, showed the relation of the pressure in the interior to the external pressure, and how this relation varied as one or other of the perforations was opened. A shallow square box of this kind was attached to the end of a square prism, and the whole was immersed with the perforated face of the box pointing upstream; it was then found that the pressure was greatest at the centre of the front, or upstream, face, of the obstacle, and that it diminished towards the edge, where, indeed, the *pressure* gave place to an outward *suction.* By attaching this box to prisms of various lengths, Dubuat showed that the greater the length of the obstacle, the less was the pressure on the front face. In order to investigate the conditions of pressure existing behind such an obstacle (which had been neglected up to that time), Dubuat turned his apparatus round (so that the perforated face pointed downstream), and proceeded as before. He ascertained that the suction on the rear of the obstacle diminished from the circumference towards the centre, and was, once again, the less considerable the greater the length of the obstacle. It had always been assumed as an axiom by previous workers in this subject that the reactions between a resisting medium and an immersed body in relative motion to it would be the same, other things being equal, whether the body moved through the stationary medium, or the medium flowed past the stationary body. Dubuat was led to question this axiom as a result of some experiments in which he towed his perforated box along under water between two flat-bottomed boats on the River Haine, near Mons, when the current had been arrested by the

closing of sluices. He compared the results which he obtained here, over a suitable range of velocities, with those derived from previous experiments, in which the water flowed past the apparatus in his artificial canal; and he thought that he detected differences, the resistances encountered in the still water of the river being systematically less than the corresponding reactions in the canal. He suggested that water at rest might be more easily divided than water in motion, and also that the layers of a liquid in motion formed a downward slope in the direction of flow, down which an immersed body would tend to slide. Dubuat recognized that a body moving in a fluid tends to carry some of the fluid along with it, the effective mass of the body being thereby increased. He sought to estimate this gain of virtual mass, in certain special cases, by studying the motions of pendulums in water. He assumed that the periods of vibration of such pendulums would not be appreciably affected by the resistance. Two simple pendulums with similar bobs vibrating in equal periods, the one in air, and the other in water, should have lengths proportional to the weights of the bobs in the respective media. But if the effective mass of the bob were increased by its carrying fluid along with it, this relation would be upset; and it was from observed discrepancies of this kind that Dubuat deduced that the mass of a globular bob in water was effectively increased by that of about half of its own volume of water, and that this relation was not greatly affected by differences in the diameters or densities of the bobs, or in the lengths of the suspensions. He also sought to obtain similar relations for bobs of other simple figures, and, by means of pendulum experiments, to compare resistances as between media of different densities. These problems of the pendulum received more fundamental treatment at the hands of Bessel in the nineteenth century.

E. Elasticity

THEORY OF BEAMS

Up to the beginning of the eighteenth century only three notable contributions to the theory of beams had been published. (1) Galilei, in his *Two New Sciences* (1638), had outlined a mathematical theory in which the load on a cantilever and the resisting force evoked in its "base of fracture" were conceived as two forces acting on the arms of a cranked lever so as to balance their respective moments about an axis which marked the intersection of the plane of the soffit and that of the base of fracture. Galilei took no account of elastic deformation, and considered the resisting force to be evenly distributed across the base of fracture. (2) Edmé Mariotte, in his

Traité du Mouvement des Eaux (1686), argued that this could not be so, as the fibres composing the substance of the beam were unequally stretched. Still clinging to Galilei's axis, he calculated the moment as two-thirds of Galilei's estimate for the same "absolute resistance," or direct tensile strength, of his material. He was led to suggest a more accurate theory (which would have made his moment one-third of Galilei's) by noting that in a simple symmetrical section half the fibres are stretched and half are compressed. But some
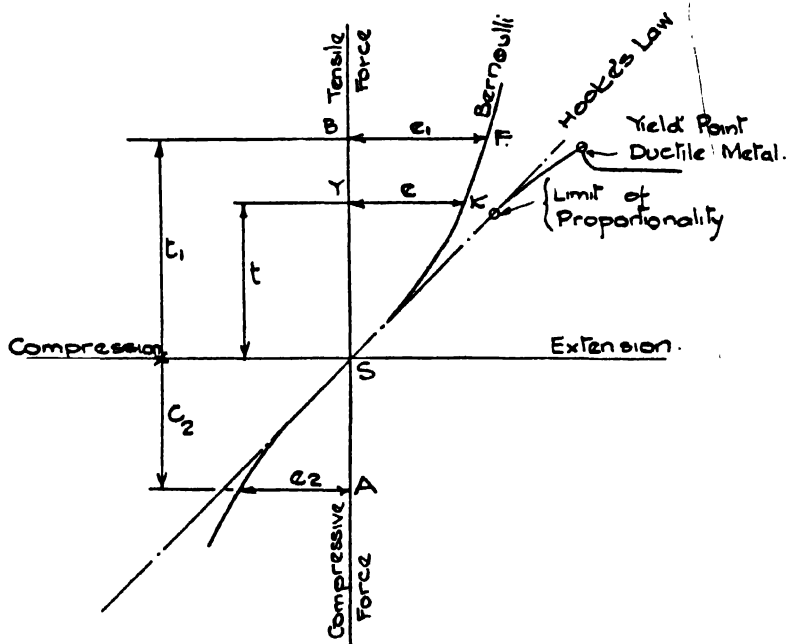
Illustr. 25.—Load-Extension Curves

rough experiments he made encouraged his adherence to the less satisfactory theory. Championed by Leibniz, this theory became established as a rival to that of Galilei throughout the eighteenth century. (3) Robert Hooke, in his Cutlerian Lecture *De potentia restitutiva* (1678), showed that applied loads and consequent distortions were in simple proportion to one another, and so he laid the basis of an approach to the subject in terms of that elastic deformation which Galilei had ignored, and of which Mariotte had failed to realize the significance. Hooke's Law, *Ut tensio sic vis* (strain is proportional to stress), was a most important step towards the solution of the problem; but it was ignored for more than a century.

The principal contributions to the theory of elasticity published

in the eighteenth century were those of Jakob Bernoulli, Euler, and Coulomb.

Bernoulli carried out some experiments on gut. Now, gut happens to behave anomalously, and so Bernoulli was misled to believe that strains increased in a lesser proportion than the stresses which produced them. This is represented in Illustr. 25, where the ratio of e_1 to e is less than the ratio of t_1 to t. Ignoring the existence of a physical limit of proportionality, Bernoulli could imagine a tensile force sufficient to double the length of a piece of material, whereas no compressive force, however great, could reduce its length to zero. Bernoulli
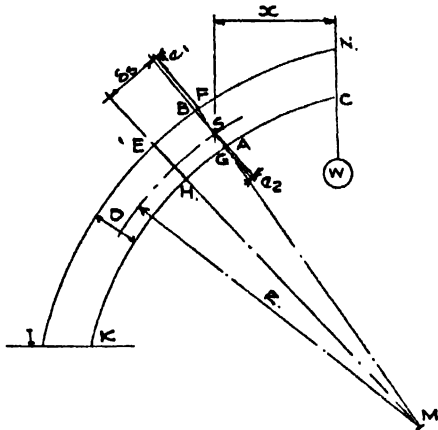
Illustr. 26.—Bernoulli's Elastic Curve

made the further mistake of disregarding the position of the *neutral* (or unstressed) layer in a beam, where compression ends and tension begins; and he thus fell back on Mariotte's unsatisfactory hypothesis. He did, however, make a most important contribution

Illustr. 27.—Bernoulli's Theory applied to the Beam

to the theory of bending by showing that the curvature of a bent member varies directly as the strains in its fibres (see Illustrs. 26 and 27). Assuming, as a case amenable to mathematical treatment, that these strains were proportional to the forces in the fibres—i.e., Hooke's Law—he obtained an equation connecting the curvature of a bent lath with the moment of the bending forces. This marked a definite advance in theory. Bernoulli's theory as later simplified by Euler amounted to this in the case of an elastic beam (see Illustr. 27): if R is the radius of curvature of the neutral axis at the point S, and S a constant depending on the elastic

properties of the material and on the dimensions of the section across AB, then the curvature $= \dfrac{1}{R} = \dfrac{W.x}{S}$. $W.x$ is the *bending moment* at the section AB for the case illustrated. S may be called the moment of stiffness of the section.

Euler applied Bernoulli's theory not only to beams but to pillars, and thus made the fundamental discovery from which all subsequent theory on the behaviour of struts has been derived. In a paper entitled *Sur la force des colonnes*, which he submitted in 1757 to the Academy of Berlin (*Mém.*, XIII, pp. 252–82), he analysed the conditions under which a long, thin, straight, elastic strut of homogeneous and isotropic material, carrying a perfectly central load, would commence to deflect. Ignoring the difference between the
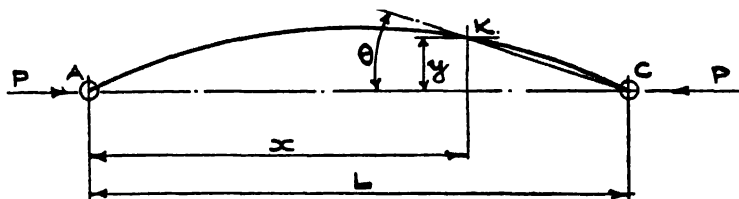
Illustr. 28.—Euler's Theory of Struts

length of the strut and the chord connecting its ends when bent, he found that,

(1) until a certain critical load was reached, no deflection whatever would occur,

(2) at the critical load the axis of the strut assumed the form of a sine curve,

(3) the strut having commenced to bend, however slightly, the load acting at the lever-arm provided by the deflection y (Illustr. 28), caused the strut to fail completely by buckling.

If P is the critical load, and L the length of the strut between the points of loading and support, then the fatal load is given by the equation $P = \dfrac{\pi^2 . S}{L^2}$ The moment of stiffness S could be divided into two parts, namely, a force, and the square of a length, making it analogous to a *Moment of Inertia*. The term *Moment of Inertia of the Section* of a beam or pillar is still used for the second moment of the area of its cross-section about a central axis; not quite in the sense in which Euler introduced it, nor so appropriately.

Euler was much puzzled by the discontinuity shown in the behaviour of the strut, especially when compared with that of a

beam like the one shown in Illustr. 29, where, he showed, the
deflection at A, produced by the force Q, may be expressed by
$\delta = \dfrac{Q \cdot l^3}{3 \cdot S}$. Any increase, however small, in the value of Q produces
a corresponding increase in the value of δ, and no critical value
appears.

In his paper on *Statical Problems applied to Architecture* (*Mém. par*
divers savants étrangers, 1773)
Coulomb supplied the first
reasonably complete dis-
cussion of the forces acting
at a typical cross-section of
a cantilever (see Illustr.
30). AD is the typical sec-
tion distant x from the line
of action of the load W. All
the internal forces acting
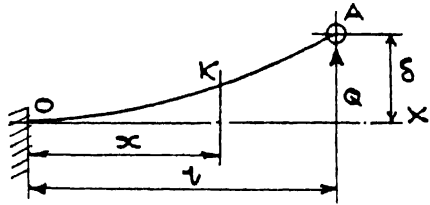
Illustr. 29.—Deflection of a Cantilever

at this section combine to resist the tendency of W to break the
beam there. The material in the upper part of the section will offer
tensile resistance, such as QP, while that in the lower part will offer
compressive resistance, such as Q'P'. Resolving those forces acting
on the portion of the beam ADKJ in the horizontal and vertical
directions, we know that in order to produce equilibrium
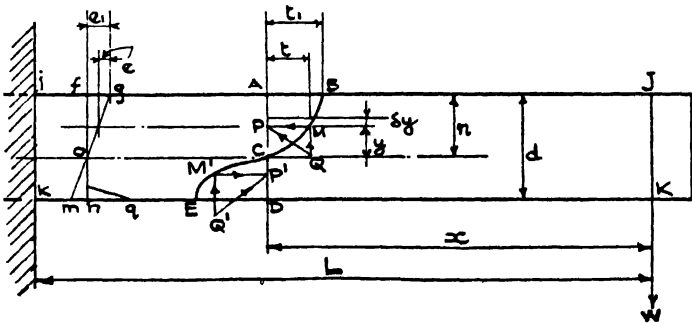
Illustr. 30.—Coulomb's Theory of the Beam

(1) the sum of the tensions MP must balance the sum of the
compressions M'P', or the area ABMC must equal the area
DEM'C,

(2) the sum of the vertical components such as QM, Q'M', etc.,
must equal W,

(3) the sum of the moments of the forces MP, M'P' about C
must equal the moment W . x, i.e., $\int t \cdot y \cdot dy = W \cdot x$.

Whatever the relationship between the distortion of the elements and their cohesion, these three conditions must be fulfilled.

Never before had this simple statement of the problem been set forth so clearly.

If the beam consisted of a piece of perfectly elastic timber, that is, one in which extensions and compressions were in direct proportion to the forces that produced them, then an element of the material at the fixed end of the cantilever, *jfhk*, would assume the wedge shape, *jgmk*, the triangle *ofg* would equal the triangle *ohm*; and (as Coulomb was considering a rectangular section) *of* must equal *oh*.

The moment of the triangle *ofg* (representing tensile forces) about

o is $(\frac{1}{2}e_1 . n) \times \left(\frac{2}{3} . n\right) = \dfrac{\quad}{3}$ and, n being $\frac{1}{2}d$, the sum of the moments of all the forces, tensile and compressive, at the section

$\dfrac{d^2}{\text{W . L.}}$

The vertical components of the internal forces such as QM were neglected, because, according to Coulomb, they have very little effect if the ratio of L to *d* is large. The recognition that the shearing forces have no appreciable effect on the bending of a long shallow beam was both important and novel.

Coulomb did, however, fall into one common error of his day: he considered that some substances, such as stone, were inextensible, and to these he considered that the theory of Galilei applied.

COULOMB'S THEORY OF TORSION

Coulomb first outlined his theory of torsion, and described experiments on the torsion of silk and hair, in a paper on the Magnetic Compass, which appeared in the *Mémoires Par Divers Savants Étrangers* for 1777. His researches on the resistance of metals to torsion were described in a paper which he presented to the *Académie des Sciences* in 1784, and which appeared in the volume of *Mémoires* for that year. In that paper he examined the relationship between the angle of twist to which a wire was subjected, and its length, diameter, and elastic properties; described the torsion balance and its use in the measurement of very small forces; and developed a surprisingly modern theory of the parts played by elasticity and cohesion respectively in the plastic yielding of metals under stress. Both papers were reproduced in Volume I of a *Collection de mémoires*, made by the *Société Française de Physique*, and published in 1884.

The experimental method adopted by Coulomb was to suspend a metal wire in a vertical position, with a heavy metal cylinder attached co-axially at its lower end. The cylinder bore a pointer
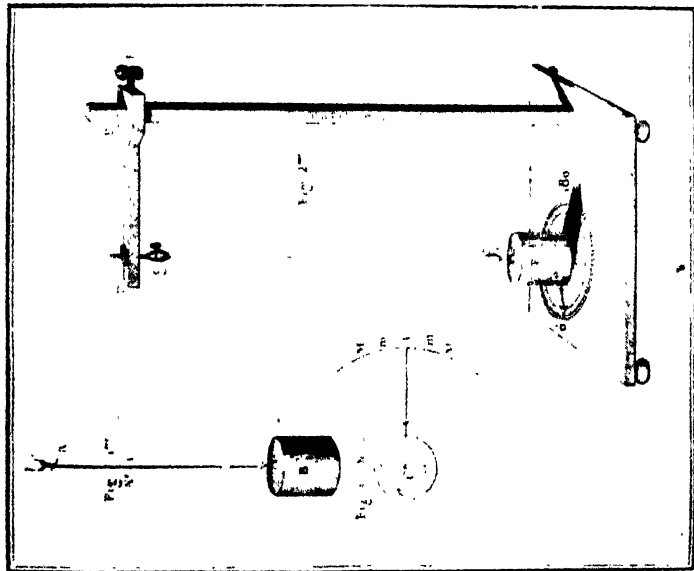
Coulomb's Torsion Apparatus
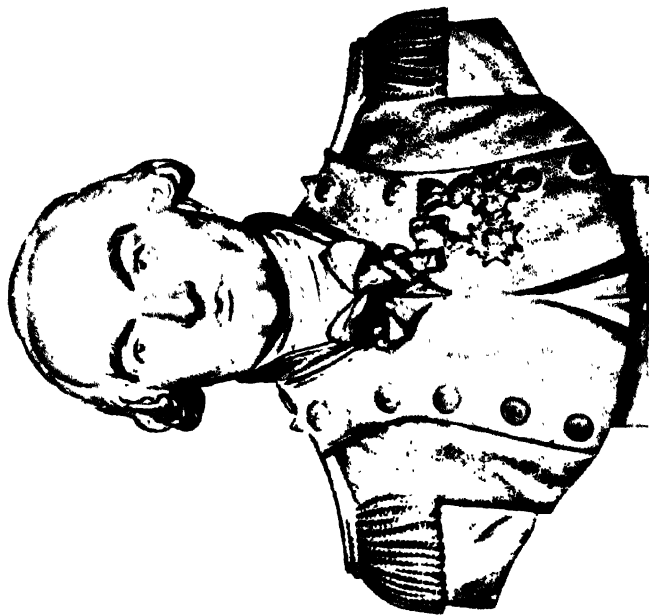
Coulomb

Daniel Bernoulli

Maupertuis

which, moving over a horizontal annular scale, registered the angle through which the lower end of the wire was twisted relatively to the upper fixed end. The weight was turned through some measured angle to twist the wire, and then released. The elasticity of the wire provided the impetus to return the system to its normal position; but, owing to the inertia of the cylinder, the pointer passed through its zero position, the wire was twisted in its opposite sense, and thus was set up an oscillatory movement which could be accurately timed.

Coulomb advanced the hypothesis that the "force of torsion" was proportional to the angle of twist. If such were the case, the cylinder suspended by the wire would be subjected to a simple harmonic motion. Let θ = the angle of displacement from the neutral position, and a = the angular acceleration of the cylinder. Let the cylinder be of mass M, and radius a, then the polar moment of inertia of the cylinder is $\dfrac{Ma^2}{2}$. The torque or twisting moment might, on Coulomb's hypothesis, be described as $n \cdot \theta$ —where n is a constant depending on some physical property of the material of the twisted wire, on its length L, and on its diameter D, in some way to be determined by experiment. We may write, then, the torque $n\theta = \left(\dfrac{Ma^2}{2}\right) \cdot a$ which gives a $\dfrac{2n\theta}{Ma^2}$

In any simple harmonic motion the periodic time is equal to 2π times the square root of a quantity obtained by dividing the displacement by the acceleration. The periodic time T for a complete cycle of oscillation is therefore given by the equation $T = 2\pi \cdot \sqrt{\dfrac{M \cdot a^2}{2 \cdot n}}$.

Coulomb did not use the terms "simple harmonic motion" or "polar moment of inertia"; but, attacking the problem from first principles, he obtained the same result, except that he gave the time for a single swing in either direction, instead of that for a complete cycle to and fro.

His investigation of the effect of damping led to a complicated formula of no practical value. We shall, therefore, neglect it, and pass on to his experiments to evaluate n.

It will be noticed that T is independent of the angle of swing. Coulomb found this to be actually the case for small angles, thus justifying his hypothesis as to the direct proportionality of torsional resistance and angle of twist. His experiments were made with iron clavicord wire, and brass wire. By using several cylinders of different weight and radius he verified the proportionality of T^2 and Ma^2, and thus, in any series of experiments in which the same cylinder was

used, he could regard T and n as the only variables. Using different lengths of wire of the same diameter and material, he found that T varied directly as the square root of the length L. Consequently, $n \propto \frac{1}{T^2} \propto \frac{1}{L}$. Using equal lengths of wire of the same material but of different diameters, he found the periodic time to be, within an error of 3 to 4 per cent, inversely proportional to the weight of wire used. The weight of a given length of wire is proportional to the square of the diameter, but is much easier to measure with accuracy.

This series of experiments gave, therefore, for a wire of weight W, but of constant length, $T \propto \frac{1}{W} \propto \frac{1}{D^2}$. Hence $n \propto \frac{1}{T^2} \propto D^4$.

If we designate μ a coefficient which depends only on the natural rigidity of the metal, the torque exerted by the wire, when twisted through an angle θ, could be expressed as

$$\text{Torque} = \mu \cdot D^4 \cdot \theta$$

The values of μ for iron and for brass wire were found to be in the ratio of 10 : 3, whereas the tensile strength of these materials, as determined by Musschenbroek, bore the ratio 5 : 3. It is not clear why Coulomb should have expected these ratios to be the same; but apparently he did, for he suggested that the difference might be due to the degrees of cold-working and annealing that the materials had undergone. A silk thread had only one-twentieth the torsional rigidity of an iron wire, which failed under the same tensile load.

Experiments on wires subjected to large angles of twist led Coulomb to note that some elastic recovery still ensued on removal of the load, even when considerable permanent set had also been given to the wire. The elasticity of metals such as iron and brass might be regarded as perfect, the forces necessary to compress or extend their component parts being proportional to the extensions or compressions suffered. It is interesting to note that, in holding this view, Coulomb subscribed unreservedly to the truth of the law enunciated by Hooke, but challenged by Jakob Bernoulli and disregarded by practically every writer during the century which separated Hooke's work from Coulomb's. The parts of a strained body were, however, joined together by cohesion, a property absolutely different from elasticity. In the first stages of torsion the component parts changed shape, lengthening and shortening without changing the points by which they adhered, because the force necessary to produce the first

degrees of torsion was less than the force of cohesion. When, however, the angle of torsion became such that the force by which the parts were compressed or extended was equal to the force of cohesion which united the component parts, they gave, separated, or slid one on another. This sliding of parts took place in all ductile bodies; but, if in the process the body was compressed, contact was increased, and the field of elasticity became greater. As, however, the component parts had a fixed shape, there was a limit to this process beyond which the body could not be strained without fracture. The essential difference between elasticity and cohesion was further emphasized by the fact that cohesion could be deliberately changed by the degree of annealing, without altering the elasticity of the material. Trying this with copper wire, Coulomb found that even when the torsional strength was reduced by half, the elastic coefficient remained unchanged. Similarly, steel rods, tested by bending under various degrees of temper, showed wide ranges in ultimate strength, but practically no variation in regard to their elasticity. This recognition of failure as due essentially to slip was ignored by Coulomb's successors, and was only revived in the twentieth century.

(See E. Mach, *The Science of Mechanics*, Tr. by T. J. McCormack, 5th edn., 1942; S. B. Hamilton, "Coulomb," in *Trans. Newcomen Soc.*, Vol. XVII, 1936–7.)

ASTRONOMY

NEWTON'S pioneer work in dynamical astronomy was followed up in the course of the eighteenth century by a group of brilliant mathematicians in France and Germany. In England astronomy was pursued mainly on observational lines. Both movements, however, have important, even spectacular, results to their credit.

A. DYNAMICAL ASTRONOMY IN FRANCE AND GERMANY

EULER

Leonhard Euler (1707-83), Alexis Claude Clairaut (1713-65), and Jean-le-Rond D'Alembert (1717-83) concerned themselves especially with the problem of determining the motions of three mutually gravitating celestial bodies. This problem is so much more complicated than that presented by two such bodies (and completely solved by Newton) as to be insoluble, in terms of known analytical functions, in its most general form. Approximate analytical solutions were arrived at, however, by these mathematicians for the particular cases of the Sun, Earth, and Moon, and of the Sun and two planets, leading to improvements in lunar and planetary theories and in the tables based on them. Thus it was with the aid of Euler's theoretical results, supplemented by numerous observations, that Tobias Mayer (1723-62) was able to construct lunar tables sufficiently accurate for use in determining longitude at sea (1755). He thus earned a prize offered by the Board of Longitude, which was subsequently paid to his widow. Euler also introduced new methods of investigating planetary perturbations. His procedure was to assume that the planet under consideration was moving, at each instant, in an elliptic orbit whose elements were undergoing slow continuous changes under the action of the disturbing planet. He then attempted to calculate the rates of change of these elements from the disturbing forces known to be at work. "Euler's Equations" are of fundamental importance in dynamics. They enabled him to predict that the Earth's axis of rotation would describe a cone about its axis of figure. The existence of a corresponding motion in the terrestrial pole was revealed in the nineteenth century as producing small periodic variations in the latitude of all points on the Earth's surface, though this effect is complicated by another, depending upon meteorological factors.

CLAIRAUT

Besides contributing to the approximate solution of the problem of three mutually gravitating bodies, Clairaut carried out a mathematical investigation of the figure of the Earth (*Théorie de la Figure de la Terre*, Paris, 1743). This was soon after his return from taking part in Maupertuis' geodetic expedition to Lapland, where results had been obtained which, taken in conjunction with those of Bouguer's expedition to Peru, tended to confirm the opinion of Huygens and Newton that the Earth is flattened at the poles. The degree of flattening revealed by these surveys, however, was about twice that which Huygens had anticipated on the ground of the Earth's rate of rotation. The problem of determining the figure of the Earth on hydromechanical principles had already led to important advances in the theory of the equilibrium of fluids. Huygens had laid down the principle that a mass of fluid is at rest only when its surface is at each point perpendicular to the resultant force acting at that point. Newton, on the other hand, had connected the state of equilibrium with the pressures acting in columns of fluid extending from the surface to the centre of attraction. The principles on which Huygens and Newton worked would have led to the same estimate of the degree of flattening of the Earth, if allowance had been made for the self-attraction of the Earth's equatorial bulge. Account of this
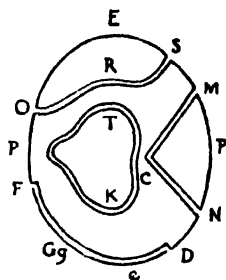
Illustr. 35.—Types of Canals

self-attraction was taken by Maclaurin and Clairaut, who used equivalent hydrostatic principles in their work. Maclaurin, developing Newton's theory, showed, in 1740, that a rotating mass of homogeneous fluid would be in equilibrium under the centrifugal and gravitational forces acting, if it assumed the form of an oblate spheroid. Clairaut, in 1743, formulated the comprehensive law that a fluid mass can be in equilibrium only when the resultant force at each point in any canal of arbitrary form in the fluid is zero. Such a canal may be conceived as arising by the remainder of the fluid becoming solid; it may form a closed circuit, or may open on to the surface of the fluid, or may lie wholly in the surface (Illustr. 35). If the fluid in each such canal is in equilibrium, the entire mass of the fluid must be in equilibrium also. From this principle Clairaut derived the partial differential equations of fluid equilibrium. He dispensed with the condition that the rotating fluid should be homogeneous throughout, and assumed merely that the layers of equal density are spheroids concentric and co-axial with the surface

of the fluid. He obtained the important relation, known as "Clairaut's Theorem," namely,

$$g_\phi = g_0 \left\{ 1 + \left(\frac{5}{2} \frac{f}{g_0} \cdot \epsilon \right) \sin^2 \phi \right\},$$

connecting g_ϕ, the acceleration of gravity in latitude ϕ, g_0, the acceleration of gravity at the equator, f, the centrifugal force at the equator, and ϵ, the Earth's ellipticity. With the aid of this result, and assuming the conditions implied in it to hold good, the Earth's ellipticity could be deduced from measurements of the intensity of gravity in different latitudes. The results of subsequent pendulum experiments in various parts of the world, however, showed many divergences from the values given by Clairaut's formula.

Clairaut, moreover, predicted, with a fair approximation to the truth, the date of the return of Halley's comet in 1759, taking into account the disturbance of the comet's motion due to the attractions of Jupiter and Saturn. He further deduced fair estimates of the masses of the Moon and of Venus (in terms of that of the Earth) from a consideration of the perturbations which they produce in the Earth's motion.

D'ALEMBERT

D'Alembert established Newton's theory of the precession of the equinoxes, and Bradley's hypothesis of the nutation of the poles, on a sound analytical basis (*Recherches sur la précession des équinoxes, etc.*, Paris, 1749).

LAGRANGE

Joseph Louis Lagrange (1736–1813) made important contributions to almost every branch of pure and applied mathematics. In a prize essay on the libration of the Moon (1764) he accounted for the equality in the periods of axial rotation and orbital revolution of the Moon on the hypothesis that the Earth's attraction upon its still liquid satellite produced in it a tidal protuberance, which is now permanently directed towards the Earth, apart from small oscillations which give rise to the physical libration. Lagrange also founded the dynamical theory of Jupiter's satellites. His greatest services to astronomy, however, lay in stimulating and supplementing the labours of his contemporary, Laplace, and in supplying him with mathematical methods of great generality which he could apply to the concrete problems of the solar system.

LAPLACE

Pierre Simon, Marquis de Laplace, was a farmer's son; he was born in 1749, at Beaumont-en-Auge in Normandy, and died in 1827. At an early age he became Professor of Mathematics at the École Militaire in Paris, and subsequently held a series of important official positions under the successive political *régimes* through which he lived. His mathematical researches were continued throughout his life, and brought him international fame and honours.

Laplace's greatest book, the *Mécanique Céleste*, which codified the work of his predecessors in dynamical astronomy, appeared, between 1799 and 1825, in five large volumes. It begins with an account of the general laws of mechanics and gravitation, and then applies these to the investigation of the motion of spheroids under their mutual attractions, the orbits, inequalities, figures and axial rotations of the planets, the oscillations and stability of the ocean, lunar theory, comets, astronomical refraction and (arising out of his theory of the latter) capillary attraction. In the fifth volume Laplace brought the work up to date with an account of his later researches.

Among the most important of Laplace's enquiries, carried out during the years 1773–84, were those concerning the stability of the solar system. In these he owed much to the general methods developed by Euler and Lagrange for investigating slow changes in the elements of a planet's orbit. Laplace found that the inequalities in the mean motions of Jupiter and Saturn, due to their mutual attraction, are periodic, and do not go on increasing indefinitely. He suspected that this might hold generally of all the planets, and verified that the secular perturbations affecting their mean motions and mean distances are, in fact, insensible. Laplace and Lagrange, working in close interdependence, followed this up by proving that the inclinations of the planetary orbits to each other must always fluctuate within narrow limits, and that a similar restriction also applies to the eccentricities of the orbits. These investigations of Laplace and Lagrange established the *durability* of the general plan of the solar system for at least an immense period of time; but as they were only approximations, taking account only of the leading terms of infinite series, they were not sufficient to prove the absolute *stability* of the system, even leaving external interfering factors out of account.

Laplace devoted a whole Book of the *Mécanique Céleste* to a detailed study of the lunar inequalities, which he deduced as consequences of the single law of gravity. He accounted for the Moon's secular acceleration, discovered by Halley, by connecting it with the slow diminution in the eccentricity of the Earth's orbit. He supposed that, since this latter change is periodic, the Moon's acceleration must

eventually be arrested and reversed. But he was mistaken, since an important part of the Moon's secular acceleration is due to tidal friction, which slowly increases the length of our day. Laplace deduced a fair value of the solar parallax from the perturbations which the Sun produces in the Moon's orbit, and the magnitude of which depends upon the Sun's distance. He further deduced a value of the Earth's (dynamical) ellipticity from the perturbations of the Moon which arise from the oblate form of the Earth. He developed an analytical theory of the tides, and, from long-continued observations of these phenomena in French harbours, especially at Brest, he deduced the mass of the Moon. He was able to disprove the common opinion that the Moon excites barometrically measurable tides in the Earth's atmosphere.

Among Laplace's other contributions to dynamical astronomy must be included an improved method for determining the orbits of comets, the discovery of remarkable numerical relations between the movements of Jupiter's satellites due to their mutual attractions, and the prediction that Saturn's Ring would be found to be in rotation.

In 1796 Laplace put forward a speculative hypothesis of the origin of the solar system from a primordial nebula. The hypothesis is formulated in his more popular *Exposition du Système du Monde* (note 7). He was trying to account for the remarkable fact that the planets all revolve round the Sun in the same direction and in planes of revolution that are very nearly identical; that the satellites also (mostly) revolve about their primaries in the same sense, and in nearly the same planes; and that the planets and satellites, together with the Sun, rotate about their axes in the same sense in which they revolve. These similarities, he argued, cannot be merely chance coincidences; they point to a common origin of all the bodies concerned.

Buffon had attempted to account for this remarkable regularity by supposing that a comet had collided with the Sun, tearing away a jet of matter from its surface, and that this matter had condensed into spheres at various distances from the Sun (*Histoire Naturelle*, Supplement Vol. V: *Époques de la Nature*). Laplace thought that this hypothesis could not account for the small eccentricities of the planetary orbits, though, in fact, the modern tidal theories of the origin of the solar system are more akin to the views of Buffon than to those of Laplace, and the difficulty arising from the high initial eccentricities of the orbits on an "encounter" theory is now satisfactorily explained.

Laplace's own hypothesis assumed that the bodies of the solar system originated from an immense, incandescent nebula, rotating

from west to east, of which the Sun is a relic. As this nebula cooled, it must have contracted, its rate of rotation increasing in the process according to the laws of mechanics. A stage would be reached at which the centrifugal force at the equator of the nebula just counterbalanced the gravitational attraction of the nucleus, and a ring of matter would then detach itself and be left behind by the contracting nebula. This would happen repeatedly, the rings so formed all lying in the same (equatorial) plane, and each rotating with its characteristic velocity. The rings being unstable, however, each would break up into rotating masses which would eventually coalesce to form a separate planet. Each of these planets would contract like the original nebula; and thus the formation of satellites and of Saturn's Ring, a satellite in the making, finds an explanation on this hypothesis. Laplace regarded the zodiacal light as another relic of the original nebula. Since the orbits of comets are highly eccentric, and occur at every inclination to the ecliptic, Laplace concluded that these bodies originated independently of the planets, and never formed part of the primitive nebula.

Laplace's nebular hypothesis was in vogue for nearly a century. It is now known to be inadmissible on the scale of magnitude of the solar system, but it roughly represents what is now thought to occur in the condensation of stars out of a nebula.

KANT

Some forty years before the publication of Laplace's nebular hypothesis an attempt had been made by the philosopher Immanuel Kant to arrive deductively at a representation of the creation of the world, and, in particular, of the solar system. In his *Allgemeine Naturgeschichte und Theorie des Himmels* (1755), Kant assumed that matter was initially distributed, in a finely divided condition, throughout the whole of space. Owing to gravitation, central bodies were formed, and also nuclei, about which the adjoining matter condensed. These nuclei gravitated towards the central bodies, but, under the influence of a repulsive force, likewise inherent in matter, they were diverted, and their fall towards the centre was transformed into a vortical motion about the centre. In this way Kant thought it possible to account for the fact that all the planets revolve about the Sun in the same sense, and in nearly the same plane. Kant's hypothesis throws no light on the origin of the *rotation* of the system. It was in order to get over this difficulty that Laplace started from the assumption of an initially rotating mass of gas, arriving eventually, as we have seen, at essentially the same result as Kant.

Kant was himself inspired in his speculations in this field by Thomas Wright's *Theory of the Universe* (1750), in which it was

pointed out that the fixed stars themselves are not scattered at random, but are condensed towards the plane of the Milky Way. Kant's hypothesis received considerable support from the fact that certain deductions which he drew from it were established by later observations. The best example of this was his computation of the period of Saturn's Ring, which he supposed had broken away from the equator of the planet. His estimate of this period, based upon elementary mechanical principles, was "about ten hours." Herschel's observations, made thirty-four years later, showed that the period is, in fact, about ten-and-a-half hours. Kant's opinion that Saturn's Ring consists of a swarm of separate particles has likewise been subsequently confirmed by mathematical, photometric, and spectroscopic investigations. From an analogy with Saturn's Ring, Kant started the theory, still partly accepted, that the zodiacal light is due to a ring of cosmic dust surrounding, and illuminated by, the Sun. He also discussed the question whether the axial rotation of the heavenly bodies could under any circumstances be diminished or destroyed. He asked whether the Moon might not have previously rotated more rapidly and have slowed down to its present rate (in exact agreement with its rate of revolution about the Earth) through the retarding action of the tidal wave raised by the Earth upon it. He showed that the rate of rotation of the Earth must likewise suffer diminution under the tide-raising forces of the Sun and Moon. Kant's surmises later received remarkable confirmation from the more rigorous investigations of Sir George Darwin.

B. Observational Astronomy in England and France

BRADLEY

The date of Bradley's birth is uncertain, but it probably fell in the year 1693. He studied and graduated at Oxford, but spent most of his youth with his uncle, the Rev. James Pound, rector of Wanstead in Essex. Pound had a little observatory of his own, and was, indeed, one of the most expert practical astronomers in England at that time. He was a friend of Newton and Halley, whom he occasionally supplied with data. From his uncle Bradley acquired an enthusiasm for astronomy, and that skill in the use of instruments which marked all his later work. The researches which Bradley carried on in collaboration with his uncle soon brought him fame; and he was elected F.R.S. in 1718 and Savilian Professor of Astronomy at Oxford in 1721. In 1725 began his fruitful collaboration with Samuel Molyneux, after whose death, in 1728, he divided his attention between his lectures at Oxford (which included experimental

physics) and his observations at Wanstead, which he continued after Pound's death in 1724. With these tasks he later combined the duties of Astronomer Royal, an office which he held from 1742 until his death in 1762.

The joint labours of Bradley and Pound were directed chiefly to the correction of Cassini's tables of the motions of Jupiter's satellites, allowance being made for the finite velocity of light. Bradley's first independent contribution to the *Philosophical Transactions* (No. 282, 1724) was an account of the comet of 1723. He was one of the pioneers in the application of Newton's method of calculating cometary orbits, which he applied successfully to this and to several subsequent comets. His most important contributions to astronomy, however, arose as by-products of an unsuccessful search for annual parallax in the stars, resulting from the Earth's orbital motion.

It had long been recognized that the detection of such parallax would be a powerful corroboration of the Copernican hypothesis. No such effect, however, had been confirmed down to the time of Bradley, though there had been several false alarms. A determined effort had been made by Hooke in 1669 to observe the parallax of γ Draconis. This star transits nearly at the zenith of a London observer, and its meridian altitude is therefore practically unaffected by refraction; but little confidence could be placed in the large value of the parallax which Hooke deduced from his observations. Fifty years later Samuel Molyneux (1689–1728), a wealthy amateur astronomer living at Kew, resolved to make another attempt upon the same star, and he had a telescope constructed for the purpose by George Graham, one of the leading instrument-makers of the period. This telescope was primarily adapted for observing γ Draconis in transit, and for accurately measuring such slight fluctuations in its meridian altitude as might arise from parallax. Bradley was a friend of Molyneux and shared in his observations, which were begun in December 1725.

The effect of annual parallax upon the apparent position of γ Draconis should have been to make the star's meridian altitude fluctuate about a mean value in a period of one year, the departure from the mean being greatest in December and in June. Bradley and Molyneux found that, in fact, the star's place showed an annual fluctuation. But they could not attribute this to parallax, for they found that the star was farthest south in March and farthest north in September, occupying its mean position in December and June. In trying to account for this unexpected behaviour of the star, Bradley and Molyneux at first suspected their instrument; but this proved to be satisfactory. They then thought that the effect might be due to a nutation of the Earth's axis: this should give rise to an equal and

opposite oscillation in a star on the opposite side of the pole from γ Draconis. The hypothesis was tested by observing such a star, selected from the few which could be observed with Molyneux's telescope. It was found to show an annual oscillation whose phase agreed with the hypothesis, but whose amplitude was too small.

Bradley now saw the necessity of extending his observations to include as many stars as possible. He therefore asked Graham to construct for him at Wanstead a telescope on similar lines to Molyneux's, but with a wider sweep, enabling some two hundred stars of Flamsteed's new catalogue to be observed in transit. With this instrument, which was completed in August 1727, he hoped to carry on parallel observations with Molyneux; but this was never done, as the Kew astronomer died shortly afterwards.

From his extended observations Bradley was able to deduce the general law that the stars showed their maximum displacements when they transited about six o'clock a.m. or p.m.; and that they moved slowly southward throughout the period when they were transiting during the day, and northward when they were transiting by night. By the end of the year 1728 he had succeeded in explaining the phenomenon by taking account of the fact that the velocity of light bears a finite ratio to that of the Earth in its orbit. "For I perceived," he wrote, "that, if light was propagated in time, the apparent place of a fixed object would not be the same when the eye is at rest, as when it is moving in any other direction than that of the line passing through the eye and object; and that when the eye is moving in different directions, the apparent place of the object would be different.

Illustr. 36.—The Aberration of Light

"I considered this matter in the following manner. I imagined CA to be a ray of light, falling perpendicularly upon the line BD; then if the eye is at rest at A, the object must appear in the direction AC, whether light be propagated in time or in an instant. But if the eye is moving from B towards A, and light is propagated in time, with a velocity that is to the velocity of the eye as CA to BA; then light moving from C to A whilst the eye moves from B to A, that particle of it by which the object will be discerned when the eye in its motion comes to A is at C when the eye is at B. Joining the points B, C, I supposed the line CB to be a tube (inclined to the line BD

in the angle DBC) of such a diameter as to admit of but one particle of light; then it was easy to conceive that the particle of light at C (by which the object must be seen when the eye, as it moves along, arrives at A) would pass through the tube BC, if it is inclined to BD in the angle DBC, and accompanies the eye in its motion from B to A; and that it could not come to the eye, placed behind such a tube, if it had any other inclination to the line BD. . . . Although therefore the true or real place of an object is perpendicular to the line in which the eye is moving, yet the visible place will not be so, since that, no doubt, must be in the direction of the tube; but the difference between the true and apparent place will be (*caeteris paribus*) greater or less, according to the different proportion between the velocity of light and that of the eye. . . . If light be propagated in time (which I presume will readily be allowed by most of the philosophers of this age), then it is evident from the foregoing considerations, that there will be always a difference between the real and visible place of an object, unless the eye is moving either directly towards or from the object. And in all cases the sine of the difference between the real and visible place of the object will be to the sine of the visible inclination of the object to the line in which the eye is moving as the velocity of the eye to the velocity of light." (*Miscellaneous Works and Correspondence of the Rev. James Bradley*, ed. by S. P. Rigaud, Oxford, 1832, pp. 6–8.)

Bradley's discovery of what is known as the "aberration of light" was communicated by him to the Royal Society in 1729 in a paper which was published in Vol. XXXV of the *Philosophical Transactions*, which was antedated 1728. The paper does not relate how the hypothesis occurred to him. The gap is filled by a story related in Thomson's *History of the Royal Society* (p. 346). The story deserves to be true, and may be quoted here as an interesting example of the value of analogies in suggesting hypotheses. Bradley, we are told, "accompanied a pleasure party in a sail upon the River Thames. The boat in which they were was provided with a mast, which had a vane at the top of it. It blew a moderate wind, and the party sailed up and down the river for a considerable time. Dr. Bradley remarked that every time the boat put about, the vane at the top of the boat's mast shifted a little, as if there had been a slight change in the direction of the wind. He observed this three or four times without speaking; at last he mentioned it to the sailors, and expressed his surprise that the wind should shift so regularly every time they put about. The sailors told him that the wind had not shifted, but that the apparent change was owing to the change in the direction of the boat, and assured him that the same thing invariably happened in all cases. This accidental observation led him to conclude, that

the phenomenon which had puzzled him so much was owing to the combined motion of the light and of the Earth."

The effect of aberration on the apparent position of any star can be calculated from a knowledge of the velocity of light, the velocity of the Earth in its orbit, and the angle between the direction in which the star appears and that in which the Earth is moving. Conversely, having measured the effect of aberration, Bradley was later able to deduce a value for the velocity of light. From his hypothesis he showed that each star must appear to describe annually a small ellipse on the celestial sphere, the major axis being parallel to the ecliptic and about forty seconds in extent, while the eccentricity depends upon the star's latitude.

While Bradley must receive the credit for discovering and ex-
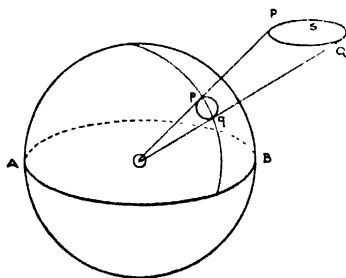
Illustr. 37.—The Aberration Ellipse

Owing to the aberration of light from the stars consequent upon the Earth's orbital motion, the apparent place of each star, S, describes annually about its mean position a circle, PQ, in a plane parallel to the ecliptic, AB. The projection of this circle upon the celestial sphere is the *aberrational ellipse, pq,* which the star appears to the observer, O, to trace out in the course of the year.

plaining the general phenomenon of aberration in the stars, it is of interest to note that the existence of such an effect had been in some measure anticipated by Römer, and that periodic fluctuations in the position of the Pole Star, presumably due to aberration, had previously been detected by Picard and Flamsteed. The apparent displacement of γ Draconis which Hooke mistook for parallax was also probably the effect of aberration.

Bradley's second important discovery was the nutation of the Earth's axis. This was the result of his prolonged study of the annual changes in the star's *mean* declination. Aberration accounted for the fluctuations in the stars' *apparent* declinations, but not for the changes in their *mean* declination. To some extent these changes might be regarded as due to precession, but not entirely, and so he sought some other explanation.

From a consideration of the manner in which the minute changes which he had observed in the declinations of the stars differed according to their longitudes and according to the position of the Moon's ascending node on the ecliptic, Bradley "suspected that the Moon's action upon the equatorial parts of the Earth might produce these effects: for if the precession of the equinox be, according to Sir Isaac Newton's principles, caused by the actions of the Sun and Moon upon those parts, the plane of the Moon's orbit being at one time above ten degrees more inclined to the plane of the equator than at another, it was reasonable to conclude, that the part of the whole annual precession, which arises from her action, would in different years be varied in its quantity; whereas the plane of the ecliptic, wherein the Sun appears, keeping always nearly the same inclination to the equator, that part of the precession which is owing
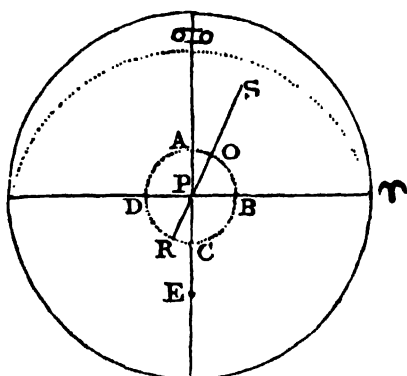
Illustr. 38.—The Nutation of the Earth's Axis

to the Sun's action may be the same every year; and from hence it would follow, that although the mean annual precession, proceeding from the joint actions of the Sun and Moon, were 50″, yet the apparent annual precession might sometimes exceed and sometimes fall short of that mean quantity, according to the various situations of the nodes of the Moon's orbit" (*op. cit.*, p. 23). Bradley's further investigations, however, convinced him that "something more than a mere change in the quantity of the precession would be requisite" (*ibid.*, p. 24) to account for all the observed facts. Bradley proposed to represent the phenomena as follows: "Let P represent the mean place of the pole of the equator, about which point, as a centre, suppose the true pole to move in the circle ABCD, whose diameter is 18 seconds. Let E be the pole of the ecliptic, and EP be equal to the mean distance between the poles of the equator and ecliptic. . . . The point P is supposed to move round E with an equal retrograde motion,

answerable to the mean precession arising from the joint actions of the Sun and Moon, while the true pole of the equator moves round P in the circumference ABCD, with a retrograde motion likewise, in a period of the Moon's nodes, or of eighteen years and seven months. . . . Let S be the place of a star, PS the circle of declination passing through it, representing its distance from the mean pole, and γPS its mean right ascension. Then if O and R be the points where the circle of declination cuts the little circle ABCD, the true pole will be nearest that star at O, and farthest from it at R; the whole difference amounting to 18 seconds, or to the diameter of the little circle" (*ibid.*, pp. 26 f.). Bradley later found that this hypothesis could be made to fit the observations better "by supposing the true pole of the equator to move round the point P in an ellipsis, instead of a circle . . . if the transverse axis, lying in the direction AC, be 18 seconds, and the conjugate, as DB, be about 16 seconds" (*ibid.*, p. 36). After-studying the relevant facts during a period of about twenty-one years, Bradley informed the Royal Society, early in 1748, that he had come to the conclusion that the pole describes a small ellipse about its mean position on the celestial sphere (*Phil. Trans.*, Vol. XLV).

Bradley took no account of solar nutation, which is of short period (six months) and quite small, and he did not attempt any thorough dynamical investigation of lunar nutation. This was undertaken shortly afterwards, however, chiefly by D'Alembert, Simpson, and Euler, whose theoretical researches suggested further unsuspected complications in the motion of the Earth's polar axis.

In all his investigations Bradley found no trace of the parallax which was the primary object of his search, and which, he concluded, could not in general exceed two seconds of arc. Aberration, however, was in its own way an equally valuable argument for the Copernican system, inasmuch as it confirmed the hypothesis of a moving Earth.

When Bradley took up his duties at Greenwich he found the Observatory in a serious state of disrepair. He had the instruments re-conditioned, but still found them unsatisfactory; and in 1748 he obtained £1000 from the Government for the construction of new and superior ones. Besides renovating the equipment of the Observatory, Bradley introduced improved methods of observing transits, and of ascertaining, and allowing for, the errors of the instruments employed. Working assiduously with his nephew and a small band of other assistants, Bradley recorded, between 1750 and 1762, some 60,000 observations, most of which were subsequently reduced by Bessel. Bradley's observations of the Moon enabled him to improve upon the best lunar tables, thus refining the methods then in use for determining longitude at sea. His other, less important,

Laplace

Bradley

Illustr. 41

Illustr. 42

Sir William Herschel

Caroline Herschel

researches included experiments on the polishing of mirrors for reflecting telescopes, an investigation of the length of the seconds pendulum in collaboration with observers in other latitudes, and the construction of more correct refraction tables. His influence also helped appreciably towards the adoption of the Reformed Calendar by England in 1752.

Outside England, the most important contributions to observational astronomy in the time of Bradley were those of the French astronomer, La Caille.

LA CAILLE

Nicolas-Louis de La Caille (1713–62) began his career as a student of theology, but he became attracted to astronomy, and joined Jacques Cassini as an assistant at the Paris Observatory. Here, and later as Professor of Mathematics at the Mazarin College, he observed the heavens with such instruments as it lay within his limited means to procure, and he wrote a number of books and memoirs. The precision of La Caille's work was in advance of the standards of the time; Delambre describes him as "*un savant qui sera à jamais l'honneur de l'Astronomie française.*" But his disposition was marked by a combination of modesty and candour which ruined his chances of advancement; and his death, at the age of 49, was hastened by overwork. La Caille was at first occupied with the extensive survey operations and the measurement of a meridian arc, which were being conducted in France at that period. Later on he led the scientific expedition which was dispatched to the Cape of Good Hope by the *Académie des Sciences* in 1750, and he was away from France for about four years. (See his *Journal historique du voyage fait au Cap de Bonne-Espérance*, Paris, 1763.) During his sojourn at the Cape, La Caille followed up Halley's pioneer explorations of the southern heavens. Some 10,000 stars were observed, of which, however, the places of only 1942 were actually reduced for inclusion in his *Stellarum australium Catalogus (Coelum australe stelliferum*, Paris, 1763). La Caille's survey embraced many new nebulae and starclusters. He measured the acceleration of gravity at the Cape and at Mauritius. He also made observations of Mars and Venus in concert with other observers in Europe; from a comparison of the two sets of observations the parallaxes of these planets, and thence that of the Sun, were deduced, though the results did not mark any advance on those based upon Richer's observations of 1672. La Caille constructed one of the best of the eighteenth-century tables of corrections for atmospheric refraction, which increases the apparent altitude of any celestial object, and the more so, the nearer the object is to the horizon. This table was based upon the results of concerted

observations of selected stars at Paris and at the Cape (*Mém. de l'Acad. des Sc.*, 1755). He tabulated, over suitable ranges of pressure and temperature, the corresponding corrections to be applied to the mean refraction at any given altitude. Towards the end of his career, La Caille published a catalogue of about four hundred of the brightest stars in the two hemispheres (*Astronomiae Fundamenta*, Paris, 1757) which, within its limits, was the finest star-catalogue prior to Bradley's. In fact, with the transit instrument which he latterly employed, La Caille was able to determine secondary star-places with an accuracy superior to that of the fundamental star-places of other contemporary observatories. He gave considerable attention to the calculation of cometary orbits. He also accumulated a mass of observations of the Sun, including a valuable series taken in the southern hemisphere where the Sun is best placed for observation in winter; these served as the foundation of his *Tabulae Solares* (1757–58).

Reference must also be made to a fellow countryman and co-worker of La Caille, namely La Lande.

LA LANDE

Joseph-Jérôme Le Francais de La Lande (1732–1807) began his career as a lawyer, but he was led to study astronomy under P. C. Le Monnier. He was sent by the Paris Academy to Berlin, equipped with a five-foot quadrant, to make observations there in concert with La Caille, who was then at the Cape of Good Hope. Berlin was chosen because of its lying as near to the meridian of the Cape as any European observatory of the time. At Berlin, La Lande made the acquaintance of Euler and Maupertuis; his results were subsequently published in the *Mémoires* of the Academy, to membership of which he was forthwith elected.

La Lande's contributions to astronomy included the measurement of the angular diameter of the Moon by means of a heliometer of 18 feet focal length, investigations (following Clairaut's methods) of the inequalities in the motions of the planets produced by their mutual attractions, and the publication in 1759 of the planetary and cometary tables of Halley, with a history of Halley's comet, which returned to perihelion in that year. He is most famous, however, as the author of the most comprehensive and most popular book on astronomy written up to his time. His *Astronomie* was first published in 1764, went through several editions, and by 1781 consisted of four large volumes, the last of which was devoted almost entirely to the tides. The first three volumes give a masterly survey of almost every aspect of astronomical study till then, including accounts of astronomical instruments and technique, astronomical measurements and computations, planetary theory and cosmology.

After Bradley's death in 1762 Nathaniel Bliss was chosen as Astronomer Royal, and upon his early death Nevil Maskelyne (1732–1811) was appointed in 1765. He had been a friend of Bradley, and had taken part in an expedition to St. Helena in 1761 for the observation of the transit of Venus foretold by Halley. A few years later he went on a voyage to Barbados with the object of testing Harrison's new chronometers. At Greenwich, Maskelyne concerned himself chiefly with observations of the Sun, Moon, and planets, recording their positions regularly with the aid of a limited number of star-places which he determined with the highest accuracy.
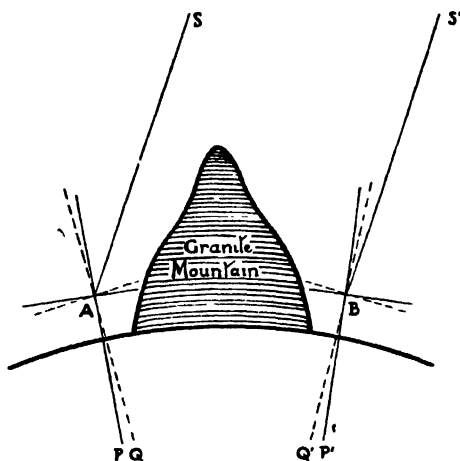
Illustr. 43.—Determination of the Earth's Density by means of Observations near Schiehallion, in Perthshire

He was instrumental in founding the *Nautical Almanac* (1767), and was its earliest superintendent.

Maskelyne made an interesting attempt to determine the mass, or mean density, of the Earth by measuring the deflection of a plumb-line in the neighbourhood of a mountain whose horizontal pull upon the plumb-bob was comparable with the downward pull of the Earth. Bouguer, while engaged in measuring a meridian arc in Peru about 1740, found that his plumb-line was deflected about 8 seconds by the attraction of Chimborazo. He arrived at a numerical estimate of the relative densities of the Earth and the mountain. This proved to be wide of the mark; but the attempt inspired Maskelyne thirty years later to undertake a similar investigation beside Schiehallion, a granite mountain in Perthshire 3547 feet

in height, chosen for its steepness and regularity of form. The principle of this investigation, the observations for which were made in 1774, is shown in Illustr. 43. A and B were stations chosen, one on the south and the other on the north side of the mountain. Careful measurements of the meridian zenith distances of selected stars were made from each station, and gave the difference in the apparent latitudes of A and B as about 55 seconds. Part of this difference, amounting to about 43 seconds, was attributable to the difference of geographical latitude of A and B, depending upon the curvature of the Earth and their distance apart, as known from survey operations. The residual difference of about 12 seconds was due to the deflections of the plumb-lines from the directions AP, BP' (which they would have assumed had the mountain not been there) to the directions AQ, BQ', consequent upon the mountain's attraction. The extent of the deflections measured the relative masses of the Earth and the mountain, and hence the Earth's mean density could be roughly computed. For this purpose, the volume of the mountain, the density of the materials composing it, and the distances of its parts from the two stations had next to be taken into account in calculations in which Maskelyne collaborated with Charles Hutton, a mathematician, and which led to the final result that the mean density of the Earth was about 4½ times that of water (*Phil. Trans.*, 1775 and 1778).

CAVENDISH

A more precise determination of the Earth's density was made by Henry Cavendish in 1797–98. The method which he employed had previously been suggested by John Michell, who had also designed the necessary apparatus. It involved the use of a torsion-balance— a device of which Michell appears to have been an independent inventor.

The apparatus consisted essentially of two small leaden balls hanging by short wires from the ends of a wooden beam suspended at its mid-point by a torsion-wire of copper, the whole protected from air currents by being enclosed in a chamber. The direction of the rod could be ascertained by means of a fixed scale and travelling vernier, read through a telescope, at each end of the beam. Two massive leaden balls were brought up outside the chamber close to the suspended balls, one on one side of the beam and one on the other, so as by their attractions to turn the rod through an angle which could be measured. Each large ball was then transferred to the other side of the beam so as to turn it in the opposite direction, and the readings were again taken. Half the difference between the two sets of readings gave the mean deflection due to the attracting

masses. In each case the system was in equilibrium under the combined action of a couple due to the attraction of the masses, and a restoring couple due to the torsion of the wire and proportional to the angle through which the wire had been twisted. By equating the expressions for these couples it was possible to express the mean density of the Earth in terms of easily ascertainable data of the experiment. Among these was the torsional constant of the suspending
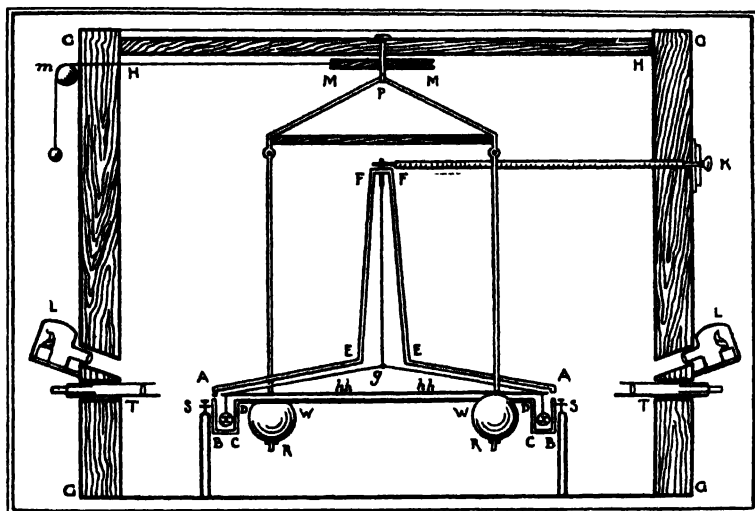
Illustr. 44.—Cavendish's Apparatus for Determining the Earth's Density

wire, which could be expressed in terms of the moment of inertia of the system, and of its period of torsional vibration, as measured in a subsidiary experiment. Substitution for these several quantities then gave the mean density of the Earth as 5·488 times that of water (*Phil. Trans.*, 1798).

The refined determination of the mean density due to Prof. C. V. Boys (described in *Phil. Trans.*, 1895) was based upon the principle of the Cavendish Experiment, and gave 5·5270 for the quantity sought.

WILLIAM HERSCHEL

Friederich Wilhelm Herschel (better known, after his British naturalization, as William Herschel) was born on November 15, 1738, at Hanover. He came of an old German family, and was the son of Isaac Herschel, an hautboy-player in the Hanoverian forces. William had a number of brothers and sisters, of whom his sister Caroline (1750–1848) played an important part in his subsequent

career. The family was poor, but the children were well educated, especially in music, so that, when just over 14, Herschel joined his father in the band of the Hanoverian Guards. At the age of 17 he had a spell of duty with the regiment in England, where he made a number of acquaintances who later assisted him. After its return to Hanover the regiment was heavily engaged in some of the battles of the Seven Years' War. Being now of military age and not wishing to serve as a regular soldier, Herschel got his discharge from the band, and, with his brother Jacob, came to England in 1757 to make a living as a musician. He had a hard struggle at first, but secured a series of posts as bandmaster or concert-manager which kept him going until 1766, when he was appointed organist at the Octagon Chapel, Bath. A few years later he brought his sister Caroline over to England to live with him.

About this time Herschel began to take a definite interest in astronomy. He had learned the principal constellations from his father while a boy at home, but he now got more systematic information from Robert Smith's *Compleat System of Opticks* (1738), and determined to construct a telescope for himself. His first instruments were refractors, the lenses of which he bought and fitted into tubes. The largest of these was 30 feet long; but they proved too cumbrous, and he turned to the more compact *reflecting* telescopes. The necessary mirrors were very dear, and Herschel set to work to grind them for himself, after the manner described in Smith's book. By 1774 he had finished a Newtonian reflector of $5\frac{1}{2}$ feet focal length, with the aid of his sister, who read to him, and sometimes even fed him, while the tedious work of polishing was in progress. With this instrument, and several others of increasing length which followed it, Herschel embarked on an examination of the Moon and planets, and, later, on a series of surveys of the whole visible heavens. He had been struck by Galilei's proposal for detecting stellar parallax by looking for periodic changes in the relative positions of close pairs of stars one of which was brighter, and hence probably nearer to the observer, than the other. With this end in view he began a search for suitable pairs of stars, of which he published a series of catalogues. In the course of this survey he made an important discovery which brought him fame and independence from his profession. On March 13, 1781, he noticed in the constellation Taurus a "nebulous star or perhaps a comet" showing an appreciable disc. He found a few days later that it had moved noticeably in relation to the surrounding stars. He still thought that it might be a comet; but Maskelyne, the Astronomer Royal, who observed the object from Greenwich, suggested that it might be a planet exterior to Saturn, as indeed it proved to be when sufficient observations had

accumulated to determine its orbit. This planet, Uranus, was the first to be discovered in historical times. Its discovery won for Herschel a Fellowship and the Copley Medal of the Royal Society, and, what was of greater consequence, brought him to the notice of the King, George III, who received him, and in 1782 made him "King's Astronomer" with a salary of £200 a year, on condition that he resided somewhere near Windsor and devoted himself to astronomy. He settled first at Datchet, but soon moved to Slough. There he remained for forty years, leading an outwardly uneventful
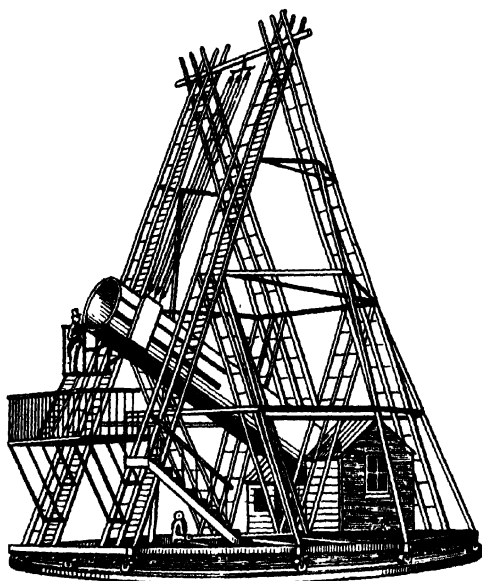
Illustr. 45.—Herschel's 40-ft. Reflecting Telescope

life, until his death, at the age of nearly 84, on August 25, 1822. Although Caroline Herschel is remembered chiefly for the devoted assistance which she rendered to her brother as his amanuensis during a collaboration of nearly fifty years, she also won distinction as an observer on her own account by her discovery of eight comets and of a number of previously unknown nebulae.

Shortly after settling at Slough, Herschel started on the construction of his largest telescope—a giant reflector of 40 feet focal length (Illustr. 45), which cost £4000 and, with the aid of ten men, took four years to complete. It is fully described in *Phil. Trans.* for 1795 (pp. 347-409). It was of the type which he described as the "Front-

view," and which is now known as the Herschelian. "It consists," he writes, "in looking with the eye glass, placed a little out of the axis, directly in at the front, without the interposition of a small speculum; and has the capital advantage of giving us almost double the light of the former constructions" (*Catalogue of One Thousand new Nebulae and Clusters of Stars, Phil. Trans.*, 1786, pp. 457 ff., note). As the observer's head obstructs some of the light, this arrangement is suited only to large instruments, and it is not common nowadays. The telescope tube was 39 feet 4 inches in length, and 4 feet 10 inches in diameter; it was made of sheet iron, the pieces being seamed together without rivets. The lower end of the tube was specially strengthened to support the speculum and to provide a pivot upon which the whole tube could turn in a vertical plane. The complicated framework of the instrument was designed to support the tube, to enable it to be elevated by means of suitable tackle to any altitude
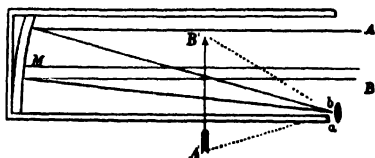
Illustr. 46.—Herschel's Reflecting Telescope in Section

The mirror was slightly tilted so that the image formed by an incident beam after reflection could be viewed through an eye-piece fixed to the upper edge of the tube, the observer turning his back to the object he was examining.

up to the zenith, and to provide the observer with convenient access to the eye-piece. The whole of the framework was mounted upon two sets of rollers running on circular brickwork tracks, so that the telescope could be brought into any azimuth. The gallery, seen in front of the telescope, would hold a number of persons; it was reached by a staircase, and could then be drawn up to any desired position. There was also a platform for the use of anyone actually observing with the instrument. The sheds served to accommodate the assistant who was to record the observations (communicated to him through a speaking-tube), and the workman who was to move the telescope when required; books, clocks, and auxiliary instruments could also be housed there. The performance of the 40-foot telescope was on the whole disappointing, as the mirror suffered distortion under its own weight of nearly a ton, and became tarnished very rapidly. The whole instrument was also very cumbrous to move.

Herschel's best work was done with a 20-foot reflector. He made notable advances in the construction of specula for such telescopes,

several hundreds of which he made for his own use, or for sale. Experience gradually taught him what combinations of metals, and what methods of polishing the castings, gave the best results.

Herschel's earliest scientific papers were read before the short-lived Philosophical Society of Bath, and dealt with a diversity of subjects— electricity, light, gravity, "Rupert's Drops," etc., besides astronomy and occasional metaphysical topics. His more mature contributions to astronomy were described mainly in papers in the *Philosophical*
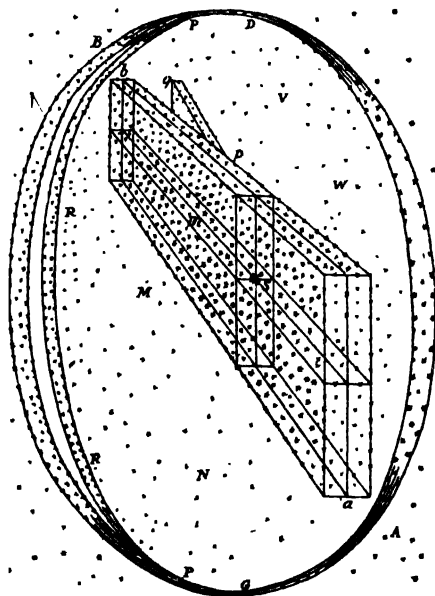
Illustr. 47.—The Milky Way according to Herschel

Transactions, dating from 1780 to 1821, and treating mainly of sidereal problems.

With the abandonment in the seventeenth century of the ancient doctrine that the stars are fixed immovably to the surface of a crystal sphere, there arose the question, how then *are* the stars distributed in space? Herschel set himself to answer this question. It was obvious that the stars are more closely crowded together in some parts of the sky (e.g., in the Milky Way) than in others. Herschel began in 1783 to measure quantitatively the density of distribution of the stars in each part of the sky by a method which he called *star-gauging*. He systematically directed his telescope towards one part of the heavens after another, and counted how many stars

he could see in the field of view at each setting, taking over a thousand such gauges in all. In some directions he could see on an average only one star at each setting, with the magnification which he was employing; in other directions he could count over five hundred. Herschel assumed tentatively that the apparent crowding of the stars in each direction might be taken as indicating the extent of the stellar system in that direction. He also assumed that the stars are for the most part of equal brightness intrinsically, so that the apparent faintness of a star is a measure of its distance from us. On these assumptions, and as a result of his star-gauging, Herschel concluded that the Milky Way consists of a stratum of stars, cloven at one end, and having the Sun somewhat displaced from its centre. (See *Phil. Trans.*, 1784, 1785.) The stars of the galactic system thus appear projected on the celestial sphere as a partially cloven great circle. Later he was led to attribute to the Galaxy a form somewhat similar to that of a convex lens or a bun: he supposed that the Sun lies in the central plane which divides the system into two halves; and in this conclusion he has been supported by refined modern investigations. Herschel realized, however, that there are regions where the stars are more closely packed than in others, and the necessity of recognizing *star-clusters* impressed itself on him as time went on. At first he classed these objects with the numerous *nebulae* of various forms which his telescope revealed in the course of his gauging operations; and he supposed that with a sufficiently powerful telescope, all nebulae could be resolved into stars. But afterwards he came to the conclusion that several essentially different types of objects are involved, and he tried to trace an evolutionary sequence through the various types, from diffuse and irregular clouds to discs which, he supposed, might give rise to stars (Illustr. 48). Concerning the objects now called *spiral nebulae*, which we see from various aspects, and which appear to be lens-shaped objects, Herschel thought that they were *island universes* comparable to the system embraced by the Milky Way. This view has returned to favour in recent years, though it has been recognized that most of the spiral nebulae are not ready-made collections of stars, as Herschel seems to have supposed, but are largely masses of gas out of which stars are still condensing, some having advanced further in this process than others.

Recognizing that the Sun is one of the stars, and that the stars are independent bodies possessing proper motions, Herschel inferred that the Sun may have a proper motion towards some point of the heavens. If the Sun alone possessed such a proper motion, all the other stars remaining at rest relatively to their mean centre, then the Sun's motion would be revealed by a gradual opening-out of the
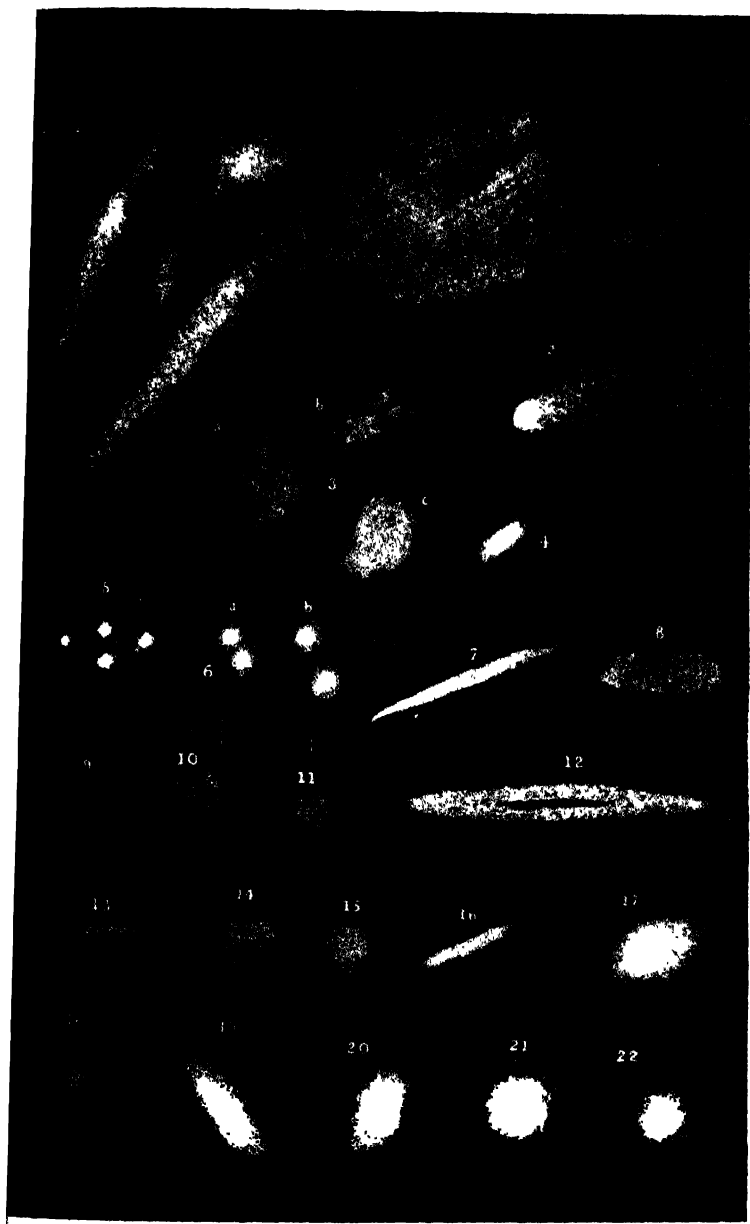
Some types of Nebulae described by Herschel

star-groups towards which he is moving, and a corresponding closing-up of those from which he is receding. The individual proper motions of the stars themselves actually complicate the matter, but if these motions are random, they may be expected to average out for a sufficient number of stars, leaving the apparent displacement arising from the Sun's motion outstanding.

Herschel in 1783 obtained a rough idea of the position of the "apex of the Sun's way" from a consideration of the proper motions of only fourteen stars, and in 1805 he obtained a confirmatory result using thirty-six stars whose proper motions had been accurately measured. He was led to assign to the apex a position in the constellation Hercules. Modern determinations of much greater refinement, aided by spectroscopic data not available to Herschel, agree roughly with his results, though the precise direction of the apex cannot be definitely determined. (See *Phil. Trans.*, 1783, 1805–6.)

From the abundance of double-stars which Herschel observed and catalogued it became clear that such a number of close coincidences between fairly bright stars could not be entirely attributed to chance; there must be some physical connection between the members of the pair in many cases. This implied that the stars of each pair, though of different brightnesses, were at very nearly the same distance and therefore were not suitable for parallactic measurements. But before Herschel's death he had succeeded in showing that, in the case of several pairs at least, the two components were revolving round each other; and after his death it was proved that this motion of the two stars about a common mass-centre was in accordance with Newton's law of gravitation, which was thus proved to hold good outside the Earth and the solar system. This has finally disposed of the Aristotelian doctrine that one set of laws holds for the Earth and a different set for the heavens. It was naturally only the more widely separated pairs of stars which could be resolved by the telescope; and these have periods of revolution often running into many thousands of years. But here again the spectroscope has, since Herschel's time, been of assistance, revealing many close doubles whose periods are of the order of only a few days.

The spectroscope, of course, was not known to Herschel; nevertheless, he was one of the pioneers in the study of stellar spectra, for in 1798 he examined several first magnitude stars through a prism fitted to the eyepiece of one of his telescopes. He obtained the spectra of these stars, noticing the preponderance of different colours in different stars, but missing the absorption lines. Another of his achievements in this domain was his discovery of the hot rays in the infra-red region of the solar spectrum (*Phil. Trans.*, 1800). Observations of Sun-spots led Herschel to the erroneous conclusion that

these objects are rifts in an incandescent atmosphere surrounding the Sun, permitting us to see a dark central globe (which he supposed might be inhabited) or else its protective blanket of dark clouds. Other minor investigations by Herschel dealt with the mountains on the Moon (whose heights he endeavoured to estimate), the periods of axial rotation of the planets, the discovery of satellites of Uranus and Saturn, variable stars, comets, etc. He also made a valuable contribution to the subsequent study of secular variations in the brightness of the stars, by compiling catalogues giving the comparative brightness of some 3000 stars, based upon his own observations. (See W. Herschel's *Collected Papers*, 2.vols., 1912; and *The Herschel Chronicle*, ed. by Lady Lubbock, Cambridge, 1933.)

GOODRICKE

A pioneer of the eighteenth century in an important department of nineteenth-century astrophysics was John Goodricke (1764–86), a deaf-mute who died in his twenty-second year. Goodricke drew attention to the regular periodicity of the light-fluctuations in the star Algol (β *Persei*), whose variability was probably known to the Arabs. Discussing the nature of this phenomenon, Goodricke wrote: "If it were not perhaps too early to hazard even a conjecture on the cause of this variation, I should imagine it could hardly be accounted for otherwise than either by the interposition of a large body revolving around Algol, or some kind of motion of its own, by which part of its body, covered with spots or such like matter, is periodically turned towards the Earth" (*Phil. Trans.*, 1783). The former of the explanations here suggested was established on spectroscopic evidence, in 1889, by H. C. Vogel, at Potsdam.

Goodricke went on to study the variability of the star β *Lyrae*, another member of the now well-defined class of "eclipsing variables." He investigated also the fluctuations of δ *Cephei*, the type-star of another important class of variables, the Cepheids.

For these researches Goodricke received the Copley Medal of the Royal Society, and he was elected a Fellow only a fortnight before his early death.

(See A. Berry, *A Short History of Astronomy*, 1898; R. Wolf, *Geschichte der Astronomie*, Munich, 1877; E. Zinner, *Die Geschichte der Sternkunde*, Berlin, 1931; H. Shapley and H. E. Howarth, *A Source Book in Astronomy*, New York and London, 1929.)

ASTRONOMICAL INSTRUMENTS

A. MAIN TYPES

WHEN the telescope was first employed in astronomy, early in the seventeenth century, it was regarded simply as a means of producing magnified images of interesting celestial objects. The manner of supporting the instrument was something of secondary importance, to be decided by the observer's convenience. The earliest telescopes were intended to be held in the hand, or to be supported in ball-and-socket bearings upon portable tripods, so that they could be directed easily to almost any part of the sky. When towards the middle of the seventeenth century the use of object-glasses of increasingly long focus demanded telescope tubes of impracticable length, some observers employed a sort of gutter of planks joined edge to edge, in the angle of which the lenses were fixed, and which was slung from a lofty mast, and manœuvred by means of ropes and pulleys. Other astronomers dispensed with telescope tubes altogether, and observed through detached lenses. Before the end of the seventeenth century, however, the need had arisen for a method of mounting telescopes which would enable them to be set rapidly upon any visible celestial body whose position was specified by appropriate co-ordinates, and when so set, to be easily capable of following that object as it was carried across the sky in the apparent diurnal motion of the celestial sphere. Meanwhile it had been recognized that the telescope, besides fulfilling valuable exploratory functions, could also be employed as a useful adjunct to the older astronomical instruments of precision, which had largely taken shape in the preceding century at the hands of Tycho Brahe. These instruments could mostly be typified by some sector of a circle whose graduated arc was traversed by a radial index, turning about the centre of the sector, and capable of being accurately directed towards any distant object with the aid of a pair of plain sights situated at its two extremities. Such an instrument could be employed to measure the angular separation of two points upon the celestial sphere (the fundamental type of astronomical measurement) by bringing its plane into coincidence with that determined by these two points and the observer's eye, directing the index towards each of the points in succession, reading the setting of the index against the graduated arc in each position, and taking the difference of the two readings as the required angular separation of the points on the sphere. By the end of the

seventeenth century, the rôle of the radial index in such an instrument had been assigned to a telescope equipped to define directions precisely by means of cross-wires in the focal plane of its object-glass. Provision had also been made for measuring the angular separations of objects simultaneously visible within the field of view of the telescope, by the invention of *micrometers*.

In accordance with the twofold function which the telescope had come to fulfil in the seventeenth century, the astronomical instruments of the eighteenth century fall into two main classes, (1) those primarily intended for making precise measurements of angular magnitude on the large scale or for the determination of time, and (2) those primarily designed for prolonged examination of the features of celestial objects and for their micrometric measurement. But the line of distinction between these two classes of instruments must not be drawn too strictly. The eighteenth century saw a wealth of interesting experiments in methods of mounting telescopes for both these types of work, and of equipping them with auxiliary apparatus of various kinds. Most of the notable astronomical instruments of the period were manufactured in London or in Paris. And whereas in past centuries the construction of such instruments had been undertaken mostly by the astronomers who purposed to use them, it was now commonly entrusted to professional craftsmen. The technical advances of the period in this realm were, in fact, mostly due to a few English mechanicians of genius, among whom Graham, Bird, the Dollonds, and Ramsden stand out prominently.

B. Some Noted Instrument Makers

George Graham (1675–1751), a Quaker from Cumberland, began his career as a watchmaker's apprentice, and succeeded in time to the business founded by his friend, and uncle by marriage, Thomas Tompion. He came to be regarded as the first mechanician of his time, both in the design and in the actual construction of astronomical and other instruments; and he was always ready to allow others to benefit by his experience. Instruments of his making which will come into our story include Halley's mural quadrant and transit instrument, and Bradley's zenith sector. In horology, Graham was the inventor of the dead-beat escapement and of the mercurial pendulum (in which the expansion of a steel rod is so compensated by that of mercury in a vessel suspended from it, resulting in a rise in the centre of gravity of the mercury, that the period of oscillation of the pendulum is unaffected by changes of temperature). John Bird (1709–76) was a Durham weaver, who took up engraving dial-plates as a hobby, and later, having studied instrument-making

under Graham, himself reached the front rank in this craft. He was especially famed for his mural quadrants. Of these, the instrument which he erected for Bradley at Greenwich in 1750 remained in service until the nineteenth century, and it was the first of a number of similar quadrants made for various European observatories. Bird was also the maker of Bradley's new transit instrument of 1750, which superseded Halley's; and he was the author of two books, published in 1767 and 1768, explaining his methods of constructing and graduating his instruments. John Dollond (1706–61), who came of a Huguenot family, started work very early in life in the silk-weaving trade, but was drawn to astronomy and optics, and eventually entered the optical trade with his son Peter. His contribution to the invention of achromatic lenses, and to their establishment in practical use (in which he was worthily followed by his son) is considered elsewhere; in the special field of astronomy he is chiefly remembered as the inventor of the most serviceable form of the heliometer. Dollond's son-in-law, Jesse Ramsden (1735-1800) came from Halifax to London to work in the cloth trade, but apprenticed himself to a mathematical instrument maker, and soon attained fame as an engraver and maker of astronomical instruments. He introduced many improvements, especially in methods of dividing scales, and in the optical side of astronomy. His name is perpetuated in the "Ramsden Eyepiece" for minimizing the defects of the images seen in a telescope (*Phil. Trans.*, 1783, p. 94). The tradition begun by these men was worthily carried on into the nineteenth century by Edward Troughton (1753–1835).

In reviewing the technical advances made possible by the skill of such men, we can only select for detailed description a few typical and historically important instruments which may serve to illustrate general tendencies in design and construction. We may begin with a group of instruments which most nearly resembled those of the seventeenth century in their subordination of the telescope to the design of instrumental structures of pre-telescopic type. These are represented by the *mural quadrants* of the great observatories, and by various types of *movable quadrants*.

C. QUADRANTS

A mural quadrant was a graduated quadrant arc of metal attached to a wall so as to lie in the plane of the meridian, with its limiting radii respectively horizontal and vertical. The classic example of such a quadrant was that erected by Tycho Brahe at his observatory of Uraniborg about 1580. The eighteenth-century mural quadrant had a telescope pivoted at the geometrical centre, and turning in

the plane, of its arc; and it was used in conjunction with a clock. Its chief function was to determine star-places. The right ascension of a star was given by the sidereal clock time of its transit across the meridian (the plane of the instrument), while, from the altitude to which the telescope had to be directed in order to take the transit, the star's declination could be deduced, allowance being made for the latitude of the observatory. On the other hand, the correct sidereal time could be obtained with the instrument by noting the instant at which a star of known right ascension transited, while the solar time was ascertained by observing the instant of
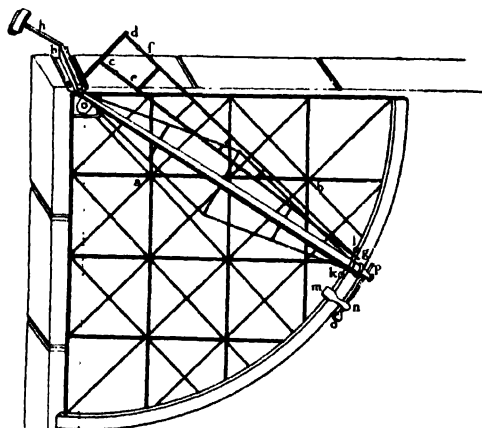
Illustr. 49.—Greenwich Mural Quadrant (1725)

the Sun's transit, and adding the Equation of Time to find the mean solar time.

Among the most celebrated of the mural quadrants of the eighteenth century was that constructed for Greenwich Observatory by George Graham in 1725 (see Smith's *Opticks*, 1738, Vol. II, pp. 332 ff.). The framework of the quadrant was composed of thin bars of iron, some having their flat surfaces placed parallel to the plane of the quadrant, while others were presented edgeways to it. These bars were all joined firmly together by means of numerous angular supports riveted at the angles between them, so as to secure the greatest possible rigidity of structure. The limb of the quadrant was composed of two quadrantal metal arcs of about 8 feet in radius, one of iron, and the other of brass partly covering the iron and partly projecting beyond it. Before erection, the brass limb was reduced to a true plane by placing it upon a level surface, and sweeping over it an iron scraper which was carried round by a

radial arm turning about a vertical axis passing through the centre of the quadrant. Two concentric arcs were then engraved upon the brass edge. The inner one was divided into degrees and twelfths of a degree; the outer one was divided into one arc of 60° and another of 30°, which were then subdivided by successive bisections into 64 parts and 32 parts respectively, or 96 parts in all, each of which was further subdivided into 16 parts. These two independent scales constituted a check upon each other, and, when settings were read upon both and reduced to the same units, the results were seldom found to differ by more than a few seconds. The quadrant was fixed to the eastern side of a free-stone wall built for the purpose in the plane of the meridian, so as to cover the southern quadrant of the meridian. Halley planned the erection of a similar instrument to cover the northern quadrant; this was begun, but the funds available were insufficient for its completion. The whole weight of the instrument was supported by two iron pins projecting from the wall, and passing through two holes in iron plates riveted to the quadrant at *a* and *b*. The pin at *a* was immovable, but that at *b* could be raised or lowered so that the bounding radii of the quadrant could be set, one horizontal and the other vertical. The plane of the quadrant, or at least the plane described by the axis of the telescope, was adjusted to coincide with the meridian by means of transit observations made simultaneously with the quadrant and with Halley's transit instrument, which stood close at hand. The quadrant, when adjusted, was kept in position by hold-fasts set in the masonry, and a plumb-line was employed to detect any progressive alteration in its situation. Nevertheless, the quadrant fell seriously out of adjustment during the latter part of Halley's term at Greenwich.

In taking a transit, the telescope was set at approximately the required altitude, the plate *mn* being clamped to the limb by tightening a screw. A fine adjustment was made, when the star had entered the field, by turning the long screw *op*, which slowly moved the eyepiece up or down the limb. The exact setting of the instrument was read with the aid of a small vernier.

A second mural quadrant, of 8 feet radius, was erected at Greenwich for Bradley by Bird in 1750; and several other mural quadrants were made in the same workshop for Continental observers. A rather unsatisfactory attempt to combine the advantages of the mural and the movable quadrants was made by P. C. Le Monnier, who in 1753 fixed a mural quadrant of 7½ feet radius, made by Bird, to a block of masonry which could be turned through 180° on castors working round a central ball-bearing.

By the end of the eighteenth century mural quadrants had very

largely gone out of favour, and were giving place to transit circles. It was found easier to divide the complete circle accurately; also errors due to incorrect centring of the circle, etc., can be largely eliminated by taking the mean of the readings of a number of microscopes distributed symmetrically round the circle.

The mural quadrant may be regarded as a particular case of the ordinary movable telescopic quadrant which could be turned into any vertical plane. Many fine instruments of this type were constructed by both English and French makers during the eighteenth century. As a typical example, we may select one made about 1770, probably by Canivet (La Lande: *Astronomie*, 1771–81 ed., Vol. II, pp. 743 ff.). This instrument consisted of an iron quadrant ABC, 3 feet in radius, with a graduated limb of copper ADB, the whole being normally free to turn in a vertical plane about a horizontal axis attached to the framework of the quadrant near its centre of gravity X. The axis was received in a hollow cylinder EE (see left lower corner of Illustr. 50); this was welded to another cylinder *e* at right angles to the first, fitting round the pin *n*, which formed the top of the base of the instrument, and about which the quadrant was normally free to turn into any vertical plane, a pointer on a horizontal circle showing the setting of the instrument in azimuth. The quadrant could be clamped in any position in altitude or azimuth, a slow-motion screw for fine adjustments in altitude being shown at B. The telescope MG was fixed by screws to lie in the plane of the instrument, with its line of collimation parallel to the radius through the 90° division on the limb. The telescope was equipped with a micrometer. From a needle passing through the geometrical centre C of the quadrant, a plumb-line was suspended crossing the limb at the graduation which measured the elevation of the telescope. The plumb-line was screened from draughts by being enclosed in a long copper box CH; one side of this could be opened like a door, and at its lower end was a lamp for illuminating the scale, and a microscope for reading the position of the thread. Provision was made for setting the quadrant in a *horizontal* plane; its axis V was inserted in the hollow cylinder S (see left lower corner of Illustr.), and the axis T was inserted in the cylinder EE, which was planted on the stand of the instrument as before.

Other movable quadrants of the period intended for use in all azimuths (especially those made in England) often let the telescope move about the centre of a fixed sector, the place of the plumb-line being taken by a vernier carried round by the telescope. J. E. Louville increased the precision of his telescopic quadrants by employing a micrometer in the eyepiece to measure the distance of

a star from the centre of the field when the telescopic sight had
been set at the nearest whole division on the limb of the instrument
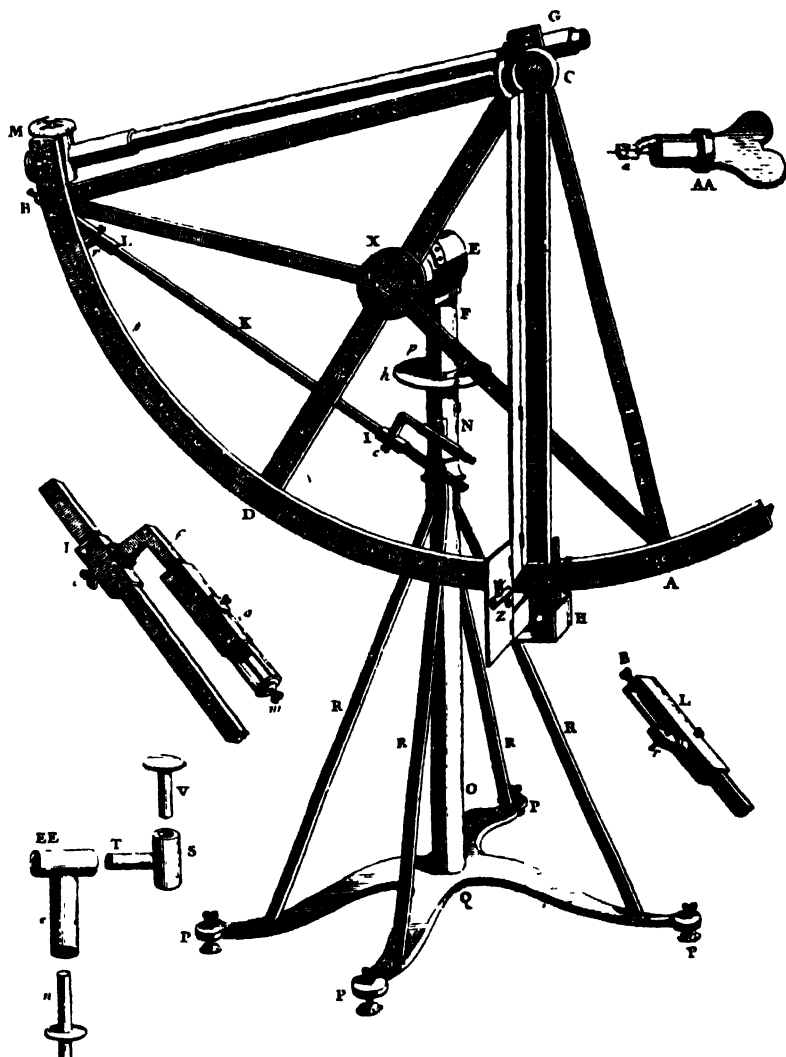
Illustr. 50.—A Movable Telescopic Quadrant (1770)

(*Mém. de l'Acad. Roy. des Sc.*, 1714). As late as 1795 Bohnenberger
made a quadrant entirely of wood, and Troughton was still con-
structing metal quadrants in the early years of the nineteenth
century.

D. Transit Instruments

A tendency to break away from these cumbrous instruments of essentially pre-telescopic design had, however, begun in the closing years of the seventeenth century with the invention of the transit instrument by Olaus Römer, about 1690. This instrument consisted essentially (as it still does) of a telescope free to turn in the meridian only, about a horizontal axis lying due East and West; it was intended primarily for measuring the right ascensions of stars whose transits across illuminated wires in the focal plane of the telescope could be timed for this purpose. In 1704, Römer went on to construct the earliest *transit circle*—a transit instrument to which was attached,
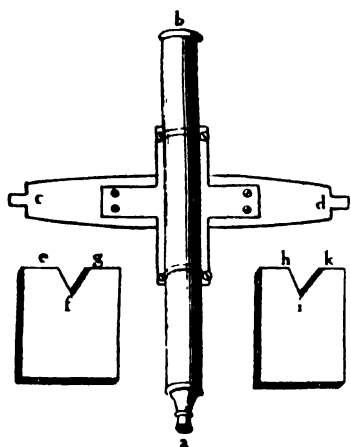
Illustr. 51.—Halley's Transit Instrument

at right angles to the axis of rotation, a complete graduated circle, which turned with the telescope, and upon which the meridian altitudes of transiting stars could be read by two fixed microscopes, and their declinations thence obtained. Astronomers were slow to follow up Römer's valuable inventions; they seem first to have been recognized in England, where in 1721 Halley had a transit instrument (though without a graduated circle) installed for his use at Greenwich Observatory.

In his *Opticks* (1738, Vol. II, pp. 321 ff.), Robert Smith describes a "meridian telescope" of the same sort (so he states) as that employed by Halley at Greenwich, though perhaps his description embodies some later improvements. There seems reason to trust the statement of eighteenth-century writers that Halley's instrument had actually been constructed by Hooke (who died in 1703). The telescope *ab* was about 5½ feet in length, and 1¾ inches in aperture; it was fixed at right angles to an axis *cd*, about 3½ feet in length, which consisted of a strong brass plate reinforced by another brass plate soldered edgeways along the back of the first. To the ends of these plates were soldered two solid pieces of brass, which were turned in a lathe to true cylinders, to form the pivots of the axis. A cross-shaped plate of brass was now firmly screwed to the axis *cd*. The upper and lower extremities of the cross were turned up at right angles to its

plane, and semicircular openings were filed in them to receive the cylindrical brass tube of the telescope, which was kept firmly in place by two brass half-collars. The bearings in which the pivots of the axis rested were V-shaped notches, *efg*, *hik*, filed in thick brass plates, one of which could be slightly raised or lowered, and the other moved backwards or forwards by screws in order to enable the axis to be made accurately perpendicular to the meridian. (The arrangement for raising and lowering one of the bearings is shown in Illustr. 52.) These plates, when adjusted, were screwed to other plates fixed firmly to masonry uprights. Halley's instrument differed from the instrument just described inasmuch as his telescope was situated nearer to one pivot than to the other. The axis of rotation of the telescope was made horizontal with the aid of a spirit-level (Illustr. 53). The tube *st* containing the spirit was

Illustr. 52.—Mechanism for Adjusting Halley's Transit Instrument

mounted on a long metal ruler having hooks *oq*, *pr*, by which the level was hung from the pivots. When this had been done, the air-bubble was brought to a definite mark near the centre of the tube *st*, by turning the screw *z*, which raised or lowered one end of the tube. The level was then reversed upon the pivots, the positions of the two hooks being interchanged, and the bubble, if no longer in its former position, was brought half-way back to it by raising or lowering one of the bearings. The new position of the bubble being noted, the level was again reversed, and any displacement of the bubble half corrected as before, and so on, until reversing the level made no difference to the position of the bubble. This hanging level was a notable innovation on earlier types. In order to make the line joining the optical centre of the object-glass to the point of intersection of the cross-wires lie perpendicular to the axis about which the telescope turned, the wires were adjusted until their intersection continued to cover the same distant mark, even when

Illustr. 53.—Hanging Spirit-level

the telescope was reversed upon its supports. When these corrections had been made, there remained the possibility that the axis of rotation of the telescope might not lie due East and West. Any deviation of this sort was detected by noting the times at which a circumpolar star crossed the meridian both above and below the Pole, and by altering the orientation of the axis of rotation until these transits took place at successive equal intervals of twelve hours. When the adjustment of the telescope was complete, the instrument was turned upon some distant object, and a mark made at the point which was just covered by the cross-wires. This served as a point of comparison enabling errors in collimation or azimuth to be subsequently detected. Halley's meridian mark (he should strictly have had two of them, as his telescope was not in the middle of the axis) was "on the park wall, near Admiral Hosier's house" (Bradley, quoted by S. P. Rigaud, *Memoirs of the R. Astron. Soc.*, 1836, IX, p. 209). In employing the telescope for night observations, it was necessary to illuminate the central wire (apparently only one was used until Bradley's time) by admitting light from a candle through a piece of horn fixed in an aperture in the side of the tube. For daylight observations, it was convenient to have a vertical graduated circle and index at right angles to the axis of rotation, so as to facilitate setting the instrument at the correct meridian altitude of a star whose transit was to be observed (Illustr. 55).

Halley's meridian telescope still hangs in the transit-house at Greenwich Observatory. It was partly superseded when Graham's mural quadrant, already described, was installed at Greenwich in 1725. An 8-foot transit instrument was later constructed at Greenwich for Bradley by Bird. It was set up in 1750, and was improved by Maskelyne, who equipped it with an achromatic object-glass in 1772 : it continued in use until superseded by one of Troughton's in 1816. By about the middle of the eighteenth century transit instruments had been installed in most European observatories.

A rather different type of transit instrument was the *instrument des passages* described by P. C. Le Monnier, in his *Histoire Céleste* (Paris, 1741)—mostly a *résumé* of the observations of the Paris astronomers between 1666 and 1686, but containing also some account of the compiler's own instruments and results (pp. lxxv ff.). Le Monnier's instrument was based upon one of Graham's construction described by Maupertuis in his *Figure de la Terre* (Paris, 1738), and was probably also Graham's handiwork; but it was designed to turn not only in the meridian but also about a vertical axis, so as to be capable of taking transits across any vertical circle, and not the meridian only. The telescope (Illustr. 54) was about 2 feet long, and had at its focal plane a web of cross-wires with adjusting
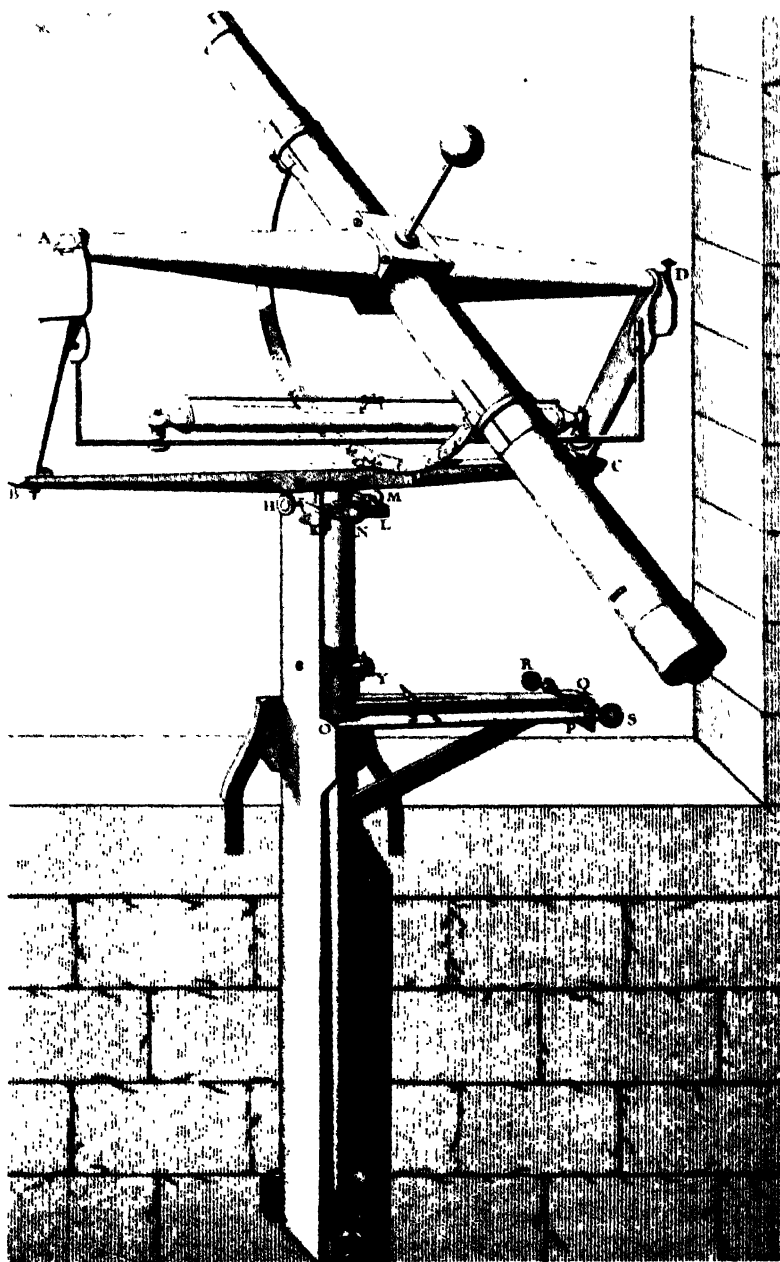
Le Monnier's Transit Instrument
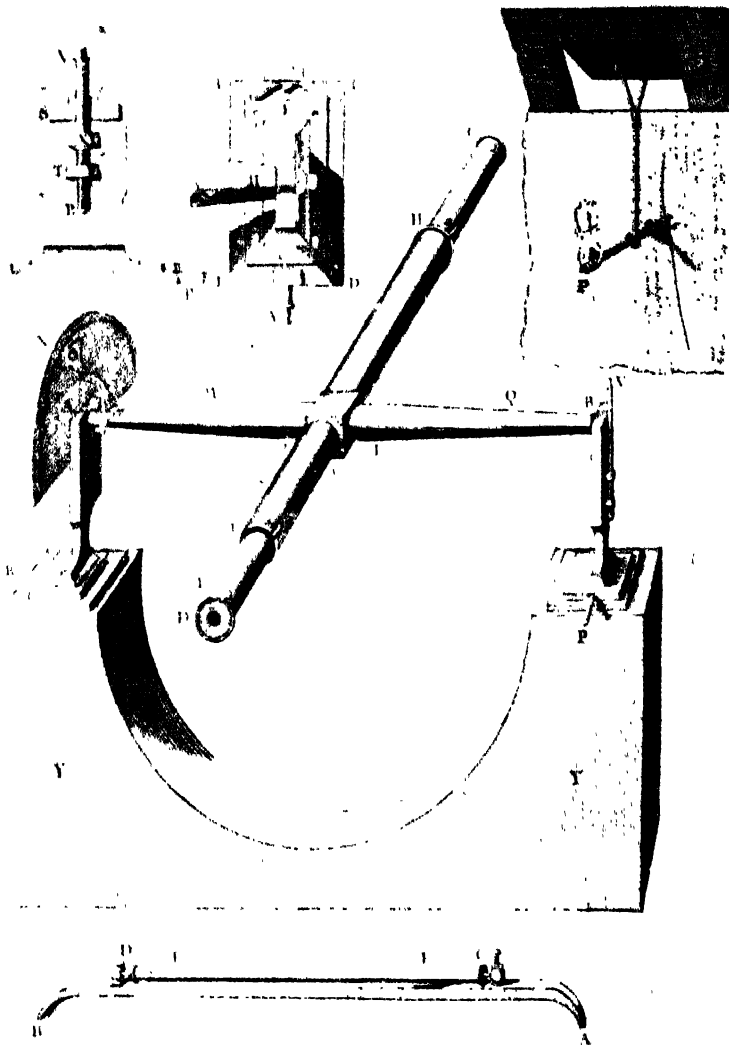
La Lande's Transit Instrument

mechanism, which was introduced into the tube through a small slot in the side, shown near the eyepiece. By means of another slot at the upper end of the tube, a ring of bright metal was supported to act as a reflector illuminating the cross-wires. The telescope tube fitted into an outer tube or jacket in which it could just turn unless clamped by tightening the rings at each end of the jacket; in this way the cross-wires could be set horizontal and vertical. The axis AD about which the telescope turned at right angles consisted of two hollow truncated cones terminating in pivots which rested in bearings at A and D. The tube and the axis were made of copper, except for the pivots, which were of harder material. The supports BA, CD, were inclined towards the observer at an angle of about 30°, so that the telescope could be set at any elevation up to 90°. These supports rested upon a horizontal brass cross-bar BC attached at its centre to a vertical iron axis turning upon a point at its lower extremity. Both the base in which this point was sunk and the collars embracing the axis were firmly connected to the supporting masonry. The axis was levelled with the aid of a spirit-level made to hang by hooks from the two pivots, and shown in position in the illustration, the procedure of reversing the level on the pivots being followed in this instrument as in Halley's. The actual levelling of the axis was effected by raising or lowering the support D with the screw shown there. The line of collimation of the telescope was rendered perpendicular to the axis by adjusting the upright wire until it covered the same distant meridian mark before and after reversing the instrument on its bearings. Any eastward or westward inclination of the upright axis to the vertical was corrected by adjusting the screws at H and M, which controlled the collar in which the axis worked; and any inclination to northward or southward was corrected by adjusting a screw at the lower extremity, which controlled the base on which the pointed end of the axis rested. In each case these adjustments were continued until, on turning the whole instrument round upon its upright axis, with the level in position, the bubble remained undisturbed. The instrument could be clamped in azimuth with the screw Y, fine adjustments of the setting in that co-ordinate being made with the screws R, S; and the graduated semicircle was an aid to setting the telescope at any desired altitude. A counterpoise to the scale is shown rising from the central cube of the instrument. Le Monnier's transit was much used for determining clock time from observations of stars at equal altitudes east and west of the meridian. The instrument proved, however, to be unsteady in use, and never became a standard design for fundamental work, though it may be regarded as an early type of altazimuth or theodolite.

Several further points of interest in connection with eighteenth-century transit instruments are exhibited in one which was probably constructed by Graham about 1730, and which was subsequently employed by La Caille and La Lande (see La Lande's *Astronomie*, Vol. II, pp. 786 ff.). The pivots of this instrument were made of an especially hard alloy of copper and tin, the bearings in which they turned being made of a somewhat softer alloy of tin and antimony. The pivots were tested for level-error by means of the striding level shown, which was applied to them from above. One of the bearings could be raised or lowered, or could be moved horizontally southward or northward, by means of adjusting screws. In the top right-hand corner of the figure is shown a contrivance for opening part of the observatory roof.

An elaborate transit circle made for F. J. H. Wollaston by Cary is described in the *Philosophical Transactions* for 1793. The circle, which was 2 feet in diameter, was read by means of moving-wire micrometers, and the whole instrument could be turned through 180° on its base by means of a winch.

An interesting experiment in the design of transit instruments, somewhat off the main track, is represented by a portable one invented by J. E. Louville (*Mém. de l'Acad. Roy. des Sciences*, 1719). In this instrument the axis about which the meridian telescope turned was itself a telescope, lying due East and West, through which the reflection of its own cross-wires could be viewed in a mirror at some distance, so as to provide a check upon any slight derangement of the axis after it had been initially adjusted with the aid of a plumb-line and of transit observations of known stars. The whole apparatus was supported upon five legs; it must have been unstable in use, and never established itself.

E. ZENITH SECTORS

The zenith sector, of which a number of examples were constructed in the eighteenth century, may be regarded as a special type of the transit circle intended for the refined measurement of small differences in the meridian altitudes of stars transiting near the zenith. It consists essentially of a long telescope turning about a horizontal axis lying due East and West near the end of the tube which contains the object-glass, while to the end near the eyepiece is fixed a scale in the form of a graduated arc. A plumb-line, hanging from the geometrical centre of the arc (which usually lies in the axis of rotation) and crossing the scale, serves to indicate the zenith distance (complement of the altitude) of the point on the meridian to which the line of collimation of the telescope is directed. The instrument is

specially suited for measuring the difference of latitude between the extremities of a meridian arc and for detecting annual parallax in a star. For such purposes, observations near the zenith have the advantage of being least affected by refraction. An instrument working somewhat on the principle of the zenith sector had been used by Hooke in his search for stellar parallax (*An Attempt to Prove the Motion of the Earth from Observation,* 1674). Picard also had used such an instrument in his geodetic work about the same period.

The first zenith sectors of high quality were those which Graham made for Molyneux (1725) and for Bradley (1727), and which enabled the latter to discover aberration and nutation. In Bradley's instrument the telescope, 12½ feet in length, was suspended by steel pins projecting from its opposite sides at the object end of the tube; these rested on bearings fixed in masonry. The setting of the instrument was shown by a plumb-line hanging from one of the pins and crossing the scale, which extended over 12½°, the accuracy of the reading being enhanced by the use of a micrometer, against the screw of which the telescope was pressed by tension in a string supporting a weight.
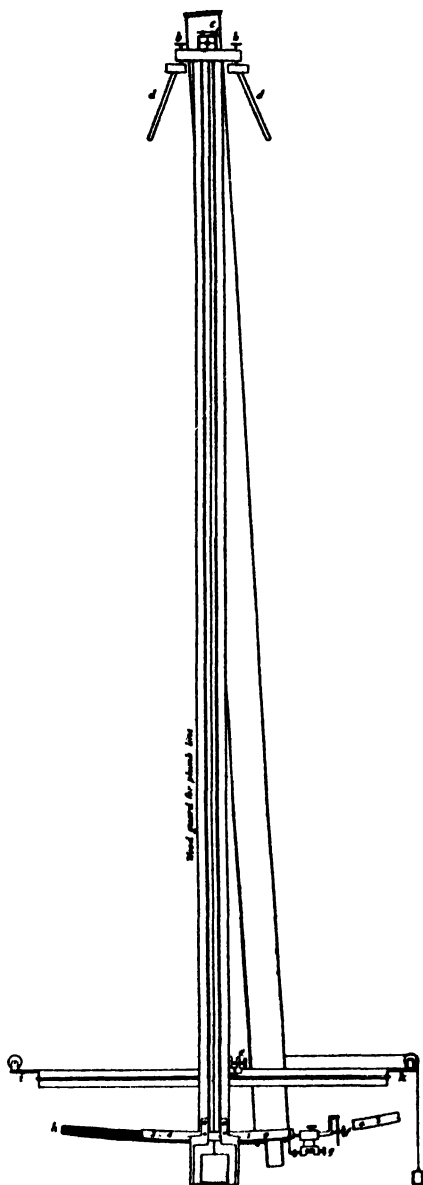
Illustr. 56.—Graham's Zenith Sector

Another notable zenith sector of the period was that employed by La Condamine in the course of his geodetic operations in South America; it is described in his *Mesure des trois premiers degrés du méridien*, Paris, 1751, and also by La Lande, from whose *Astronomie* the following account and illustration are derived (1771-81 ed.,
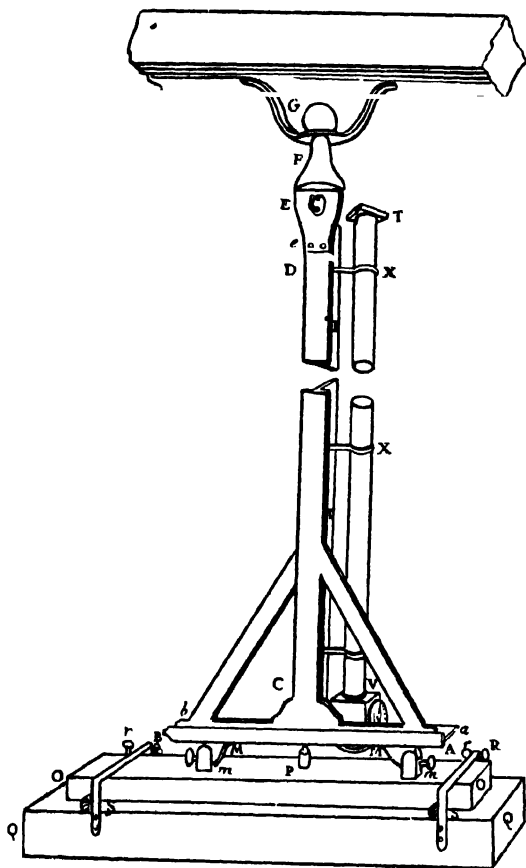
Illustr. 57.—La Condamine's Zenith Sector

Vol. II, pp. 781 ff.). The telescope VT was securely attached to a radius CDG free to turn about G in the plane of the meridian through small angles with the vertical, and carrying at its lower extremity a cross-piece AB on which was a graduated arc of about 8° engraved on a copper limb, and traversed by a plumb-line EP suspended from a pin at the geometrical centre E of the arc. The

radius measured 12 feet from C to D; it was composed of flat iron bars fixed at right angles to each other to secure rigidity; and it was surmounted by a piece of copper EFG, carrying the pin E from which the plumb-line was suspended, and ending in a globular head G, the neck of which worked in a collar attached to a firmly supported cross-beam. The radius was thus supported with some measure of freedom, and the observer had a clear view of the zenith. The cross-piece AB was 2 feet in length; two tongues M, M, projected downwards from it and fitted into grooves in the metal blocks m, m. These blocks were fixed in a stout beam OO, which rested on a firm foundation QQ, and could be displaced in the direction of its length (northward and southward), thereby tilting the telescope through a few degrees either way from the zenith. When the telescope had been set approximately at the required zenith distance, the beam OO was clamped by screws r, r, and the fine adjustment was made with the screws m, m. The actual setting of the telescope was shown by the graduation at which the plumb-line crossed the arc, and additional precision was secured with the aid of the micrometer V.

TELESCOPES FOR OBSERVING EQUAL ALTITUDES

Several eighteenth-century telescopes were specially designed for determining the times when the Sun, or a star, was at equal altitudes
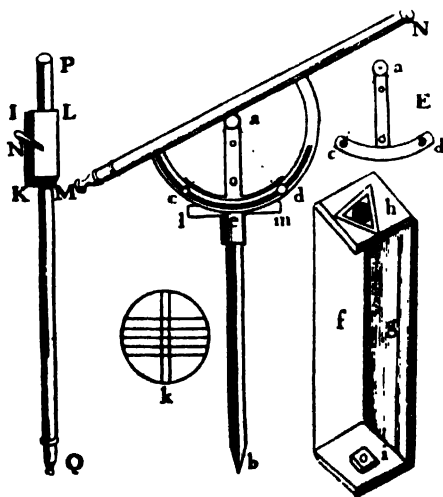
Illustr. 58.—Telescope for Observing Equal Altitudes

before and after its passage across the meridian, the mean of these times giving the actual time of meridian transit. P. C. Le Monnier's

instrument des passages, already described, was frequently put to this use. Another such instrument, described by Robert Smith (*Opticks,* Vol. II, p. 329), consisted essentially of a telescope MN, 30 inches in length, supported on a steel axis *ab* of the same length; this axis stood upright in a sort of oblong box, firmly fixed to a pillar, and it was free to turn upon its pointed lower end *b,* which rested in a bearing at *i,* while its cylindrical portion *e* turned in a collar at *h.* To this axis was attached a small metal arc *cd* of 60° (shown at E) whose centre *a* was at the top of the axis *ab.* The telescope was fixed to the diameter of a metal semicircle, having the same radius and centre as the metal arc. The semicircle was normally free to slide over the arc, to which it was held by the two screws *c, d,* which passed through a slit in the semicircle and entered holes in the arc; but, by tightening these screws, the telescope could be clamped at any desired elevation above the horizon. The axis *ab* was made vertical by moving the base *i* with screws until the spirit-level *lm* remained horizontal as the axis was slowly rotated. The telescope, when clamped in any position, then behaved as an almucantar, sweeping out a circle of constant altitude. A star was selected east of the meridian, and was followed with the telescope clamped in altitude, the star being kept between the two vertical wires in the field of view (*k*), and the times of its passage across each of the five horizontal wires being recorded. When the star, having crossed the meridian, again came into the field of the telescope, still at the same setting, the times of its passages across the five wires were again taken, and the mean of all the ten times was adopted as the time of the star's meridian transit.

F. Equatorial Telescopes

We turn next to methods of mounting telescopes applicable in cases where it was desired to set the instrument readily upon some celestial object, and to keep that object in the field of view some little time, for observation or micrometric measurement. This was effected by mounting the telescope so that it could turn about an axis at right angles to its own length, while this axis, with the telescope attached to it, turned about a second axis perpendicular to the first one. With these degrees of freedom, the telescope could be directed to any point on the celestial sphere (except, indeed, where its motion was obstructed by portions of the supporting structure, as frequently occurred in the earlier types of mounting). Among the special cases covered by this general description, the one which principally concerns us here is that in which the telescope turns in a plane perpendicular to the Equator, about an axis lying in the Equator and turning about a second axis parallel to that of

Illustr. 59
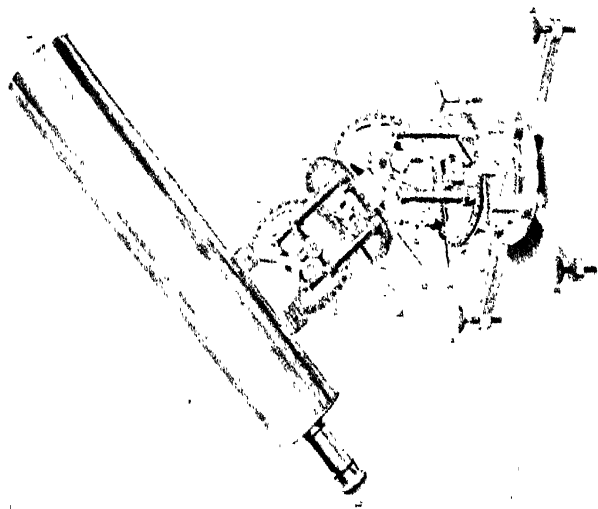
Short's Equatorial Telescope

Illustr. 60

Nairne's Equatorial Telescope

the Earth's diurnal rotation. The readings on the graduated circles by means of which the telescope is set will here represent the declination and the hour-angle of the star.

Several anticipations of this *equatorial* form of mounting are to be found in seventeenth-century astronomy. Christoph Scheiner, in his *Rosa Ursina* (1630, pp. 347 ff.) describes such a mounting (which he attributes to Christoph Grienberger); a telescope intended for forming images of the Sun upon a screen turned about a polar axis, and was free to move over a graduated arc of 47° in declination, so as to be able to follow the Sun throughout the annual cycle of its declination. Again, Robert Hooke, in his *Animadversions on the First Part of the Machina Coelestis* (London, 1674, pp. 67 ff.; reprinted in Gunther's *Early Science in Oxford*, Vol. VIII), describes, not only an equatorial mounting for a quadrant, but also a contrivance of his own invention "by means whereof the Quadrant being once adjusted, and set to the Objects, will continue to be so, for as long a time as shall be desired, without at all requiring the help of any one hand of the Observator." Flamsteed's equatorially mounted sextant at Greenwich was probably constructed under the influence of Hooke's ideas. In the closing years of the seventeenth century, Römer's observatory on the Round Tower of Copenhagen could show an example of an equatorially mounted telescope complete with divided circles and reading microscopes (P. Horrebow: *Basis Astronomiae*, Havniae, 1735, Ch. VI). The equatorial mounting was employed also by the Cassinis (*Mém. de l'Acad. Roy. des Sciences*, 1721).

The majority of the eighteenth-century equatorials were portable instruments. A mounting of this kind, intended for a reflecting telescope, was described by James Short in the *Philosophical Transactions* (1749, p. 241). The graduated circle AA was first made horizontal by means of the four screws B, B, B, B, and with the aid of two spirit-levels attached to it. The circle DD was then brought into the plane of the meridian by turning the screw C, and the circle FF was brought into the plane of the Equator by turning the screw E. In setting the telescope upon a given star, the circle HH and the telescope itself (a Gregorian reflector) were brought into the plane of the star's hour-circle, as indicated on FF, by turning the screw G, and HH was then turned by the screw K until it was set at the star's declination, when the setting was complete. Below the circle AA can be seen a magnetic needle; this could be used for approximately orientating the telescope, or alternatively for measuring the magnetic declination. A somewhat similar instrument to Short's, but more stable, was later described by Edward Nairne (*Phil. Trans.*, 1771, p. 107). (See Illustrs. 59, 60.)

La Lande, in his *Astronomie*, describes an equatorial instrument of

the contemporary type (1771 ed., Vol. II; Illustr. 61). The axis CY of the instrument was supported by a wooden stand which was kept in a horizontal position by means of three levelling-screws, and with the aid of a pair of spirit-levels at right angles to each other. The portion BKN of the base lay in the meridian, and the axis CY was inclined to it at an angle equal to the latitude. This axis turned with gentle friction in a copper collar at Y; its lower extremity C fitted into a hemispherical bearing, and it terminated above Y in two jaws which enclosed a metal semicircle VZ; this turned about a pin passing through its centre S, or it could be clamped when desired. To the diameter of this semicircle was screwed a grooved wooden support, which was to receive the telescope, and the limb of the semicircle was graduated, a vernier enabling the setting of the telescope in declination to be read to 5 seconds. Some instruments were equipped with a slow-motion screw I. The setting of the telescope in hour-angle was shown by a pointer attached to the axis, and traversing the semicircle OC parallel to the plane of the Equator. In constructing the instrument, the inclination of its polar axis to the horizontal was made equal to the latitude in which it was to be used, but the instrument could be adjusted for use in a slightly different latitude by tilting it in the plane of the meridian through a corresponding angle, as shown by the displacement of the plumb-line against the graduations on the arc R, whose centre r was the point from which the line was suspended.

Most of the equatorials of this period were subject to the drawback of being so constructed that they could not be turned so as to point to the Pole, nor, indeed, sometimes to any star within about 30° of the Pole. Megnié's equatorial (La Lande, *op. cit.*, Vol. IV, p. 666, and pp. 669 f.), made in 1774, was free from this disability. The telescope VL and the declination-circle IK lay at opposite ends of the declination-axis HX, and the instrument could quickly be adapted for use in any latitude, the movable semicircle FG being turned through the angle requisite for setting the circle EQ in the plane of the Equator. In observing elevated objects, however, there would be little room for the observer's head (see Illustr. 62).

Another advance in the design of equatorially mounted telescopes was represented by the instrument constructed in 1791 by Ramsden for Sir George Shuckburgh (*Phil. Trans.*, 1793, p. 67). The telescope and its declination-axis were sustained by six tubular brass supports, joined at their upper ends to a circular frame from which the upper pivot rose, and resting at their lower ends on a conical pivot which supported the weight of the instrument. The telescope was free to turn through a complete revolution on its declination axis (Illustr. 63).

The equatorial mounting of reflecting telescopes presented special

Megnié's Equatorial
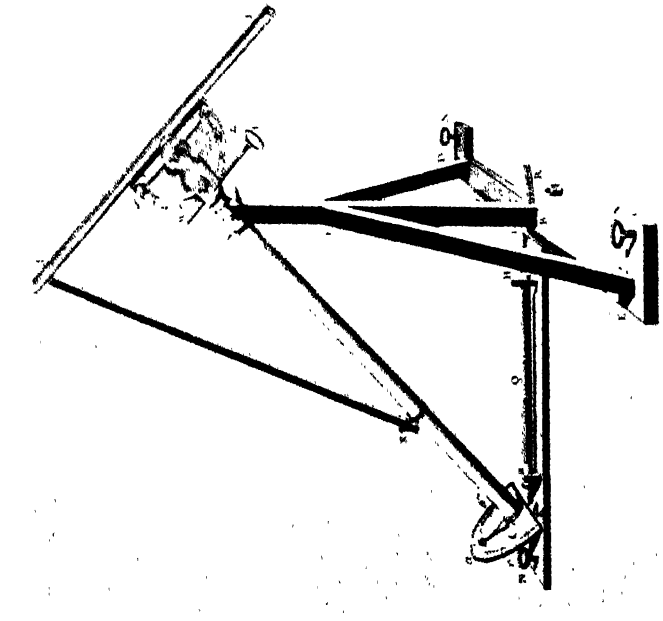
An Equatorial Instrument of 1770
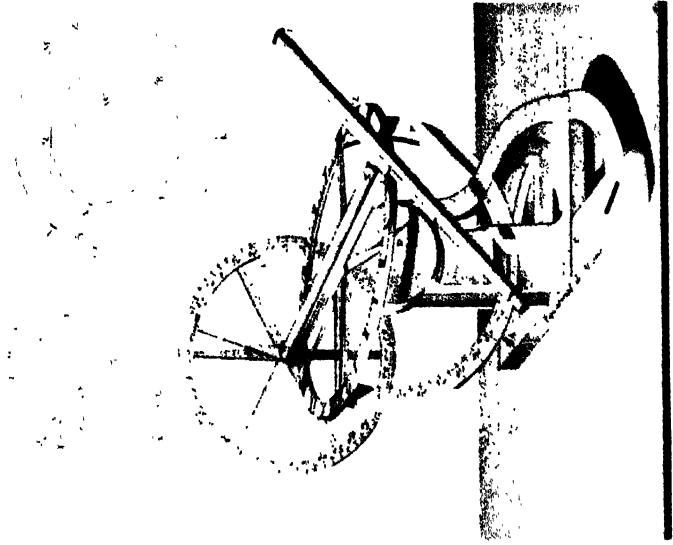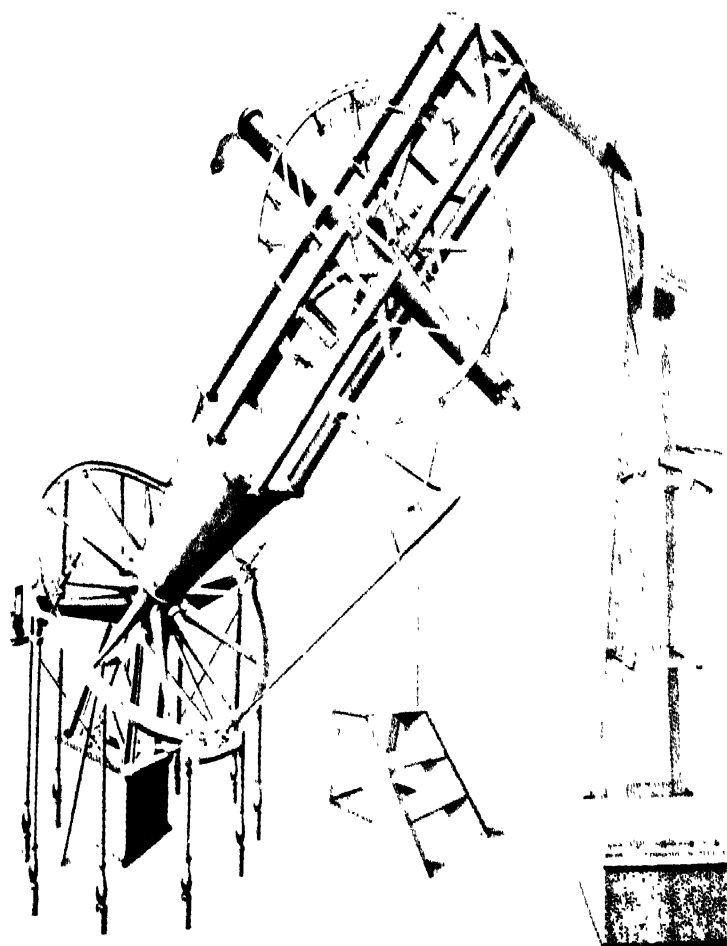
Ramsden's Equatorial

problems. Progress was made in methods both of manufacturing and of mounting these instruments by John Hadley (see Smith's *Opticks*, Vol. II, pp. 305 ff., and 366 ff., and *Phil. Trans.*, 1723, Vol. XXXII,
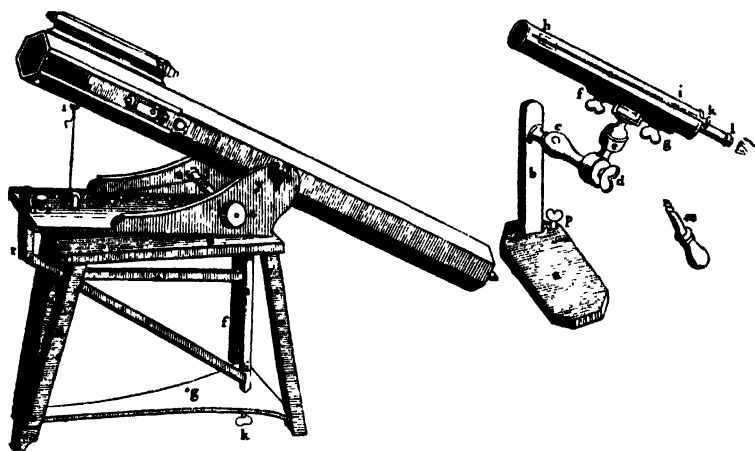
Illustr. 64.—Hadley's Mounting of Reflecting Telescopes

p. 303), and by Passement (*Description et usage des Télescopes*, 1763). But all eighteenth-century advances in this field were overshadowed by the achievements of Sir William Herschel (see Chapter IV).

G. ASTRONOMICAL SECTORS

Before turning to consider eighteenth-century devices for measuring the smallest observable celestial angles, we may note a type of instrument devised by Graham for determining differences in the right ascensions and declinations of pairs of celestial objects not sufficiently close together for their separations to be measured with a micrometer (Smith's *Opticks*, Vol. II; Illustr. 65). This instrument, known as an "astronomical sector," was mounted upon a square axis HIF set parallel to the Earth's polar axis, and ordinarily free to turn upon a pin entering the conical hole H, while the cylindrical portion I of the axis rested between the two prongs of a metal fork. The circular brass plate *abcd* was fixed parallel to the axis HF; and about a joint F at the centre of this plate there turned a brass cross KLMN, two of whose arms were bent up at right angles at O and P to support the radius CD of the graduated arc AB (comprising 10 or 12 degrees). The rotation of the axis HF in right ascension, and the rotation of the sector about F in declination, could be arrested when desired by means of clamps acting on the circle Q and the

cross MN. About the centre C of the arc AB the telescope EC was free to turn (carrying its attached vernier along the divisions of the sector) under the action of the slow-motion screw *g*. In employing this sector to determine the differences in the co-ordinates of two stars, the radius CD was clamped at such an inclination to the polar
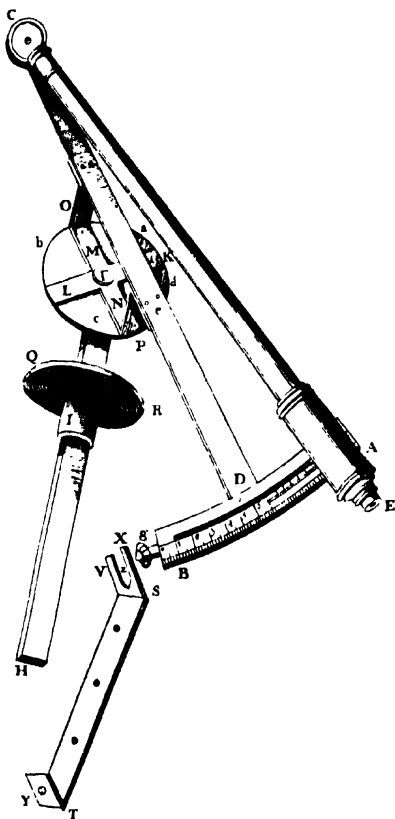
Illustr. 65.—Graham's Astronomical Sector

axis that both stars could be observed with the telescope somewhere within the extent of the arc AB, without the necessity of unclamping in declination; differences of declination greater than the angle ACB could not, therefore, be measured with the instrument. The plane of the sector was then clamped in right ascension a little to the westward of both stars, and their transits over this plane were observed. The

difference between their times of transit gave their differences of right ascension, and the differences of the settings of the telescope (measured on the arc AB) required to bring the successive stars to the centre of the field gave the difference in their declinations.

H. MICROMETERS

Considerable progress had been made during the seventeenth century in the construction of *micrometers*. The general form which these important adjuncts to the telescope had assumed at the hands of Picard, Auzout, and Römer, and the functions which they were designed to fulfil, are summed up by Robert Smith in the following terms: "A micrometer is a small piece of mechanism, contrived for moving a fine wire parallel to itself in the plane of the picture of an object formed in the focus of a telescope, and with great exactness to measure its perpendicular distance from a fixed wire in the same plane: and the use of this instrument is to measure small angles subtended by remote objects at the naked eye" (*Opticks*, Vol. II, p. 342).

One of the most successful of the eighteenth-century micrometers working on the traditional lines, is shown in Illustr. 66. (See Smith: *op. cit.*, Vol. II, pp. 345 ff., and La Lande: *op. cit.*, Vol. II, pp. 768 ff.) This instrument, which was probably designed by Graham, consisted of an oblong brass plate AB in which was cut an aperture *abcdef* across which was passed a horizontal wire *be*, and two slender upright bars *gh*, *ik*, of which *gh* was fixed, while *ik* could be moved sideways in either direction by turning the micrometer screw CED, which was provided with indices to show upon the dial EF the number both of complete revolutions, and of parts of a revolution, of the screw, corresponding to the separation of the bars *gh*, *ik*. The whole instrument was intended for insertion in the focal plane of a telescope, and the original design was improved upon by Bradley, who, instead of attaching the instrument directly to the telescope, mounted it upon a brass base (represented by LN in the lower figure) upon which it could be turned, in its own plane, about the intersection δ of the wire *be* and the fixed bar *gh* as centre, by turning the endless screw *στ*, which engaged a toothed sector attached to the base LN; it was this base which was attached to the tube of the telescope by means of the projections shown along its sides. By this device the orientation of the micrometer could be altered without turning the telescope about its axis as a whole. La Lande was still using a micrometer of this type in 1771, and he regarded it as the best available for general purposes.

It was necessary to calibrate such a micrometer so as always to

be able to convert the separation of two star-images, expressed in turns and fractions of a turn of the screw, into the angular measure of the arc separating the stars upon the celestial sphere. The usual method of effecting this (already followed in the seventeenth century, and still employed to-day) was to set the micrometer wires perpendicular to the Equator, to separate them through a known and
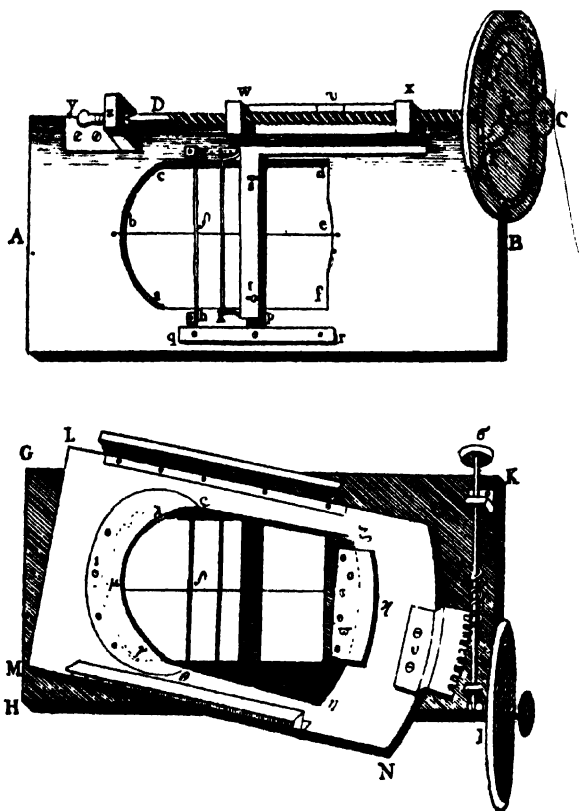
Illustr. 66.—Graham's Micrometer (front and rear views)

preferably fairly large number of turns of the screw, and to observe the time taken by the image of some known star near the Equator to pass from one wire to the other, subsequently converting this time-interval into angular measure, with allowance for the slower apparent motions of stars of greater declinations. Alternatively, a micrometer could be calibrated by comparing the separation of two marks on a distant wall, as measured by the instrument, with

the angle actually subtended by the marks at the observer's eye, as calculated by their distance apart and from the place of observation.

In working with such a micrometer, it is frequently necessary to adjust the orientation of the intersecting wires in the field of view, so that one or other of these passes through two selected star-images, or lies parallel to the direction in which the stars drift across the field. In several of James Bradley's micrometers (as described by Smith, *op. cit.*, Vol. II, pp. 344–45), such adjustments were facilitated by mounting the wires upon a ring *abc*, free to turn in its own plane in a circular groove cut in a larger concentric ring ABC, which was fixed in the focal plane of the telescope. The wires are indicated by *gh*, *ik*, *ln*. The inner ring, which was held in the groove by projections at A, B, and C, could be rotated into any desired position by turning the screw DEF, which engaged a toothed sector *de* screwed to the inner ring.
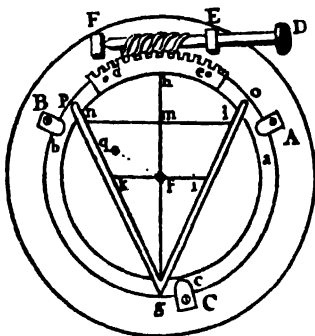
Illustr. 67.—Bradley's Micrometer

The brass bars *go*, *gp* were fixed to the inner ring, and were intended to be of service in determining the difference of declination of two close stars. They were inclined at such an angle that the perpendicular distance *mf* between the paths *ln*, *ik*, of two stars across the field, should be equal to the difference (*ln–ik*) of the portions of these paths intercepted between the bars. Hence the difference of *declination* of the stars could be easily deduced from the difference of the *times* taken by them to pass between the bars.

I. Heliometers

A type of micrometer working on an entirely different principle was invented in the middle of the eighteenth century, and was named the *heliometer*, because it was first employed to measure the angular diameter of the Sun. The effective invention of this instrument was due to Pierre Bouguer, though it was afterwards found that the principle of the device had been anticipated by Servington Savery; and even Römer seems to have had the idea of making a telescope with two movable object-glasses (J. B. Du Hamel: *Histoire*, 1701, p. 148). Bouguer described his invention in the *Mém de l'Acad. Roy. des Sciences* (1748, p. 11); and Illustr. 68 shows an early form of the instrument (see La Lande: *op. cit.*, Vol. II, pp. 811 ff.). The heliometer is shown attached to the object end of the telescope

tube A, whose section is given by the dotted line RRR. It consists essentially of two lenses (or lens-segments), B, E, of equal apertures and equal focal lengths; of these, B is fixed, and E is attached to a chassis FGHI, which can be moved towards or away from B by turning the micrometer screw NMLO. A moving index I, and a fixed scale P, enable complete turns of the screw to be reckoned, and the micrometer scale M shows fractions of a turn, so that the separation of the two object-glasses can be accurately evaluated. The instrument behaved like two telescopes with one eyepiece, and, in measuring the diameter of the Sun, two images of that body, ST and RV, could be formed in the common focal plane AAA. The two glasses were then adjusted until the two images touched at T, when the required angle was obtained by dividing the separation of the centres of the two lenses by their common focal length, expressed in the same units. La Lande devised means of adjusting the lenses without the observer having to leave the eyepiece (*Mém. de l'Acad.*, 1754, p. 597).

When James Short heard of Bouguer's invention, he was reminded of a suggestion made to the Royal Society some years before by Servington Savery. He obtained Savery's original memoir from Bradley, and it was published in the *Philosophical Transactions* (1753, p. 165). The memoir had been read to the Society by Bradley on October 27, 1743. Savery's proposal was to determine the *difference* between the apparent diameters of the Sun, when at perigee and at apogee respectively, by forming two images of the Sun side by side, with a small interspace, and measuring this interspace with a micrometer when the Sun was at the two apses of its apparent orbit. Accuracy was to be obtained by magnifying the Sun's disc highly, taking into the field only the essential portions of the limbs of the two images. Savery proposed to obtain his double images by dividing a lens into segments along parallel chords and sticking corresponding segments together with paper in various ways (he suggested a similar procedure with mirrors), or, preferably, by mounting two lenses of equal focal lengths side by side, and viewing the images of the Sun which they formed through a single eyepiece. However, Savery does not seem to have got beyond the experimental stage in the construction of such instruments.

The idea was next taken up by John Dollond, whose heliometer marked an improvement on those of Savery and of Bouguer. In Dollond's heliometer, which became the standard form of the instrument, an object-glass was divided along one of its diameters into two halves; these could be equally and oppositely displaced through measurable distances in the line of this diameter, each half of the lens forming its own complete image in the common
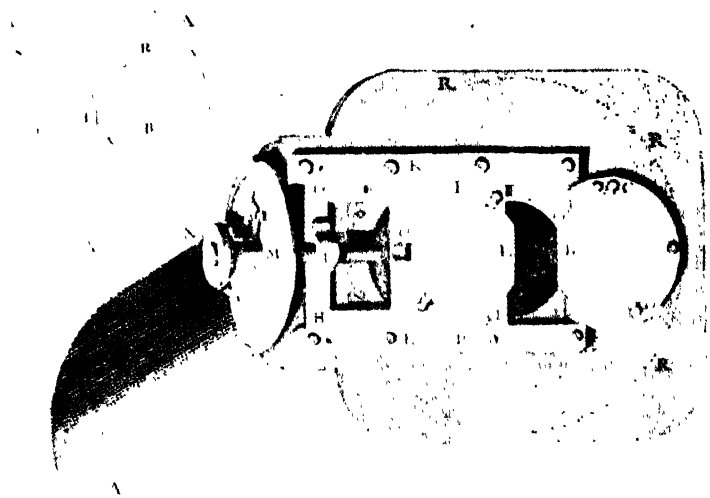
Bouguer's Heliometer
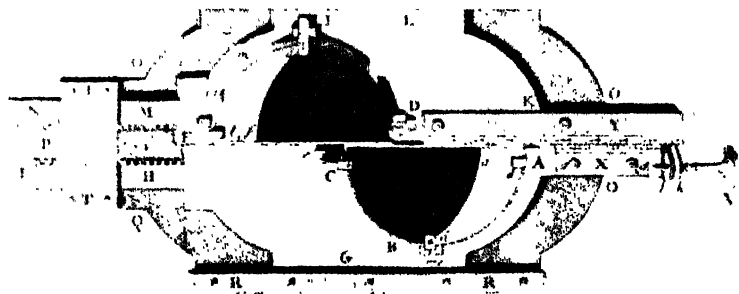
John Dollond's Heliometer
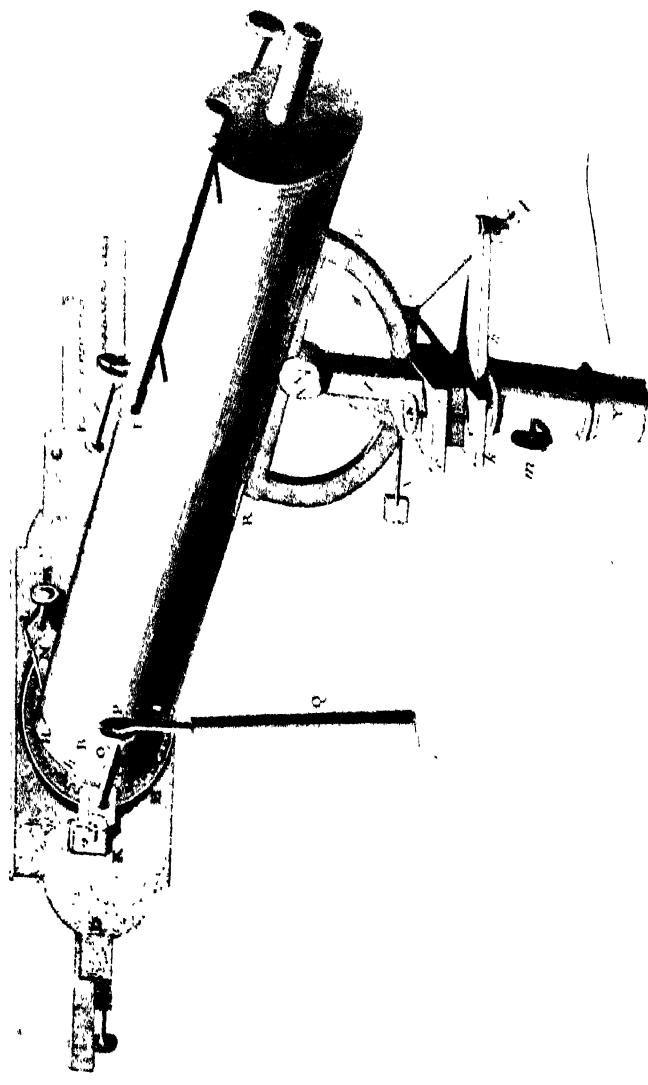
Heliometer attached to a Reflecting Telescope

focal plane (*Phil. Trans.*, 1753, p. 178; and 1754, p. 551). An early form of Dollond's instrument is shown in Illustr. 69 (La Lande: *op. cit.*, Vol. II, pp. 814 ff). The dotted line LaBG*f* represents the aperture of the telescope, and ABC, DEF, are the two halves of the object-glass, which move parallel to AF. (The portions of the object-glass falling within the aperture of the telescope are shaded *dark*.) The segment ABC is fixed to the frame AGHI, which is of copper, and the segment DEF is fixed to the frame KLMN. These frames end in racks HI, MN. A pinion near P, turned by a handle with a universal joint, engages these racks, moving them equal amounts in opposite directions, and the separation of the lenses can be read to one-five-hundredth of an inch by means of the scale Y and the vernier X. Dollond's instrument could be applied to a reflecting telescope. It is seen so employed in Illustr. 70. The handle PQ served to turn the dividing diameter of the lens into any desired direction across the field, provision being made for reading the position-angle of this diameter.

Over and above its original function of measuring the diameter of the Sun, the heliometer came to be used for determining the angular diameters of the planets, the separations of binary stars, and the positions of planets relative to the background of stars; and it played a valuable part (notably in the hands of Bessel) in the discovery of stellar parallax early in the nineteenth century. The heliometer had the advantages over ordinary micrometers that precise settings could be made with it despite irregular guiding of the telescope; that it could be applied to measure the disc of the Sun or Moon even when the magnification was so great that only small portions of the images appeared in the field of view; that contacts between images were observed at the very centre of the field, where the conditions of observation were best and that no illumination of cross-wires was called for. During the past century, however, the heliometer has largely fallen into disuse. It was expensive to construct and tedious to manipulate, and it demanded great skill in the observer. Most of its functions have now been taken over by photography.

(See J. A. Repsold: *Zur Geschichte der astronomischen Messwerkzeuge*, 1908.)

MARINE INSTRUMENTS

A. The Nautical Sextant

THE fundamental methods of determining position at sea involve the measurement of the altitudes of known celestial bodies above the horizon, or of their positions relative to the neighbouring stars, by means of instruments capable of being effectively employed on board ship. Ever since the rise of ocean navigation in the fifteenth century, sailors. had relied for this purpose upon a variety of such instruments whose antecedents, in some instances, went back to medieval or ancient times, but which were gradually modified and improved in the light of experience. Of these contrivances, the cross-staff, the astrolabe, the sea-quadrant, and the back-staff, were the chief representatives. Their general function was to fix, in the vertical plane through a selected celestial object and the place of observation, the direction of the horizon and the direction of that object, by the alignment of pairs of sights, and thereupon to enable the angle between these directions to be deduced from the setting of the instrument. All previous devices of this class, however, were superseded early in the eighteenth century by the invention of an instrument which soon assumed the familiar form of the modern *nautical sextant*.

The credit for the effective invention of this instrument, which Arthur Schuster called "the most perfect appliance that has ever been invented," must be shared between two men—John Hadley (1682–1744), a mechanician of genius who became Vice-President of the Royal Society, and Thomas Godfrey (*d.* 1749), a self-educated glazier of Philadelphia, who belonged to Benjamin Franklin's intellectual circle. It subsequently transpired, however, that the instrument, in all essential respects, had been invented years before by Newton. Moreover, a somewhat similar, though inferior, instrument had been described and constructed even earlier by Robert Hooke.

HOOKE

It appears that Hooke, on August 22, 1666, mentioned to the Royal Society a "new astronomical instrument for making observations of distances by reflection," which at the Society's bidding he constructed, and submitted on September 12th following (Birch's

History of the Royal Society of London, 1756–7, Vol. II, pp. 111–4). This was probably the instrument to which Waller alludes in his *Posthumous Works of Robert Hooke* (1705, p. 503), where the following passage occurs: "I shall here add the description of an instrument for taking angles at one prospect, as I found it described upon a loose paper, *ee*, *ff*, two long rulers, or arms, opening upon a joint or centre *g*, *hh*, a ruler divided into a thousand parts, measuring the angle at *g* by a table of chords; *ab* a telescope fixed on the ruler *ff* so that the middle of it may lie perpendicular over the inner edge of the ruler; *a* the place of the cross-sight; *b* the object-glass; *i* the eye-
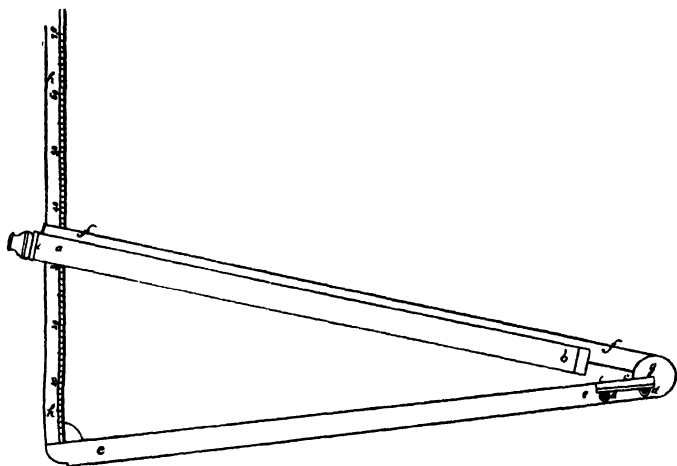
Illustr. 71.—Hooke's Reflecting Instrument

glass; *cc* the reflex-glass, whose edge just touches the centre *g*, and whose surface *cc* is in the same plain [*sic*] with that of the inner edge of the ruler *ee*; on the back side of which glass is a brass plate, with two ears *dd*, at right angles, by which it is screwed to the ruler *eo*." Hooke's instrument thus employed only one reflecting mirror instead of two; this would save some loss of light, though the reflected image would be somewhat diffuse when the incidence was very oblique. But the instrument had the disadvantage, if used to measure, say, the altitude of a star above the horizon at sea, that when it was correctly held and set the sea would be entirely hidden behind the reflector, and the star would be at the limit of the field of view.

NEWTON

It was Newton who seems first to have proposed the addition of a second reflecting mirror, which is the essential feature of Hadley's

instrument. At what date Newton made this suggestion is not known; and with his characteristic indifference to his own discoveries, he had been dead for fifteen years, and Hadley's invention had become established, before it leaked out, almost by accident, that Newton
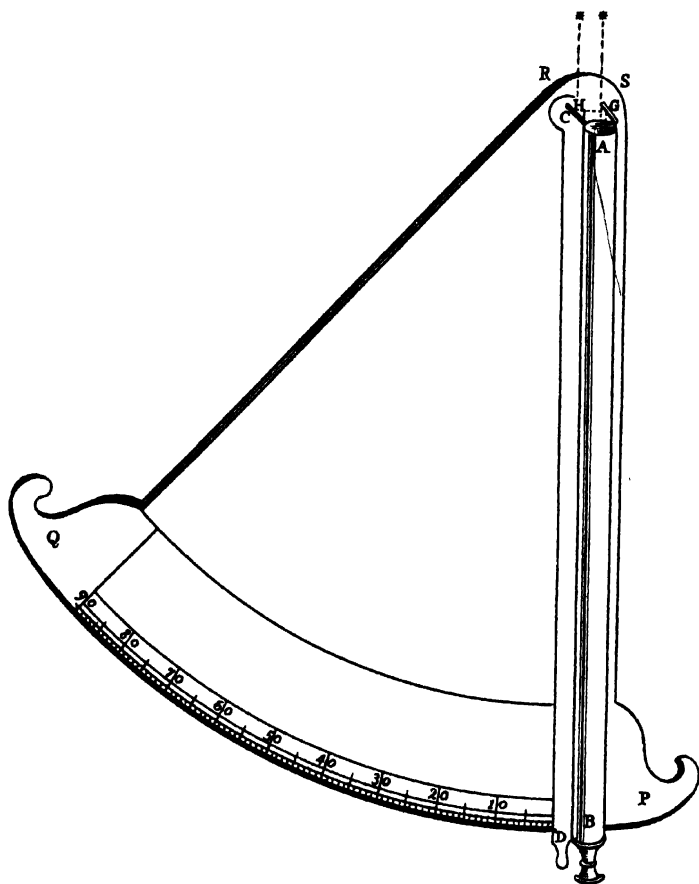
Illustr. 72.—Newton's Sextant

had made any contribution to the problem at all. Hadley's account of his invention, indeed, immediately awakened in Halley the memory of something which Newton had once suggested; and a search of the records at Halley's instance revealed that Newton had communicated, in 1699, a suggestion for improving on the traditional

type of sea-quadrant. This, however, was seen to have no relevance to what was now proposed by Hadley. Halley admitted that he must have been mistaken concerning the nature of Newton's proposal. But Halley had been right after all, for after his death in 1742 there was found, apparently mislaid among his papers, an account in Newton's handwriting of an instrument which Newton had designed, but which, to judge from the wording, he does not seem actually to have constructed. This memorandum was printed in the *Philosophical Transactions* (1742, p. 155), and the essential portion runs as follows: "PQRS denotes a plate of brass, accurately divided in the limb PQ . . . AB is a telescope, 3 or 4 feet long, fixed on the edge of that brass plate. G is a speculum fixed on the said brass plate, perpendicularly, as near as may be, to the object-glass of the telescope, so as to be inclined 45° to the axis of the telescope, and intercept half the light which would otherwise come through the telescope to the eye. CD is a movable index turning about the centre C, and, with its fiducial edge, showing the degrees, minutes, and $\frac{1}{4}$ minutes, on the limb of the brass plate PQ; the centre C must be over-against the middle of the speculum G. H is another speculum, parallel to the former, when the fiducial edge of the index falls on 00° 00' 00"; so that the same star may then appear through the telescope in one and the same place, both by the direct rays and by the reflexed ones."

HADLEY

The first account of John Hadley's "new instrument for taking angles" was read before the Royal Society on May 13, 1731, and was subsequently published in the *Philosophical Transactions* (Vol. XXXVII, p. 147). Two alternative methods of construction were indicated. The first of these is described as follows: "The instrument consists of an octant ABC, having on its limb BC an arch of forty-five degrees, divided into ninety parts, or half-degrees, each of which answers to a whole degree in the observation. It has an index ML movable round the centre, to mark the divisions; and upon this, near the centre, is fixed a plane speculum EF perpendicular to the plane of the instrument, and making such an angle with a line drawn along the middle of the index, as will be most convenient for the particular uses the instrument is designed for . . . IKGH is another smaller plane speculum, fixed on such part of the octant as will likewise be determined by its particular use; and having its surface in such direction, that when the index is brought to mark the beginning of the divisions (i.e., 0°), it may be exactly parallel to that of the other: this speculum being turned towards

the observer, and the other from him. PR is a telescope fixed on one side of the octant, having its axis parallel to that side, and passing near the middle of one of the edges IK or IH of the speculum IKGH, so that half its object-glass may receive the rays reflected from that speculum, and the other half remain clear to receive them from a distant object . . . ST is a dark glass fixed in a frame, which turns on a pin V; by which means it may be placed before the speculum EF, when the light of one of the objects is too strong." The second of Hadley's alternative methods of constructing his instrument, shown in Illustr. 74, consisted in placing the telescope across the radius of the octant; and this is the form of the instrument which has been adopted. The surface of what we should call the index-glass coincided with the fiducial line of the index. Metallic specula were at first used, but as these were found to tarnish, silvered plates of glass were substituted, the silvered half of the horizon-glass IKGH lying nearest the plane of the instrument. (The third mirror NO was for use when angles of more than 90° were to be measured, and W, Q, were open sights which were alternative to, or interchangeable with, the telescope.) Hadley's instruments had the ad-

Illustr. 73.—Hadley's First Sea-Octant

vantage, in practice, over Hooke's and Newton's, that the images of two points whose separation was required could be brought into contact along that edge of the horizon-glass IKGH which was parallel to the plane of the instrument, the two images being thus brought into coincidence while still in full view.

In using the instrument to measure the angular separation of two distant objects, say two stars, the telescope was pointed towards one of them, and the plane of the instrument was set so as approximately to pass through them both, with the mirror EF lying on the same side of IK as the other star lay. The index was then turned until the image of the second star, formed by light successively reflected from EF and IK, appeared in the field with that of the first, formed by direct light, and ultimately coincided with it. From optical considerations it followed that the angular separation of the two stars (in *degrees*) was then given by the reading (in *half-degrees*) upon the

limb. Similarly for measurements of the altitude of the Sun's lower limb above the apparent horizon. Correct settings could be made with the instrument even when it was unsteadily supported. It was therefore especially suited for use at sea. Moreover, in the older
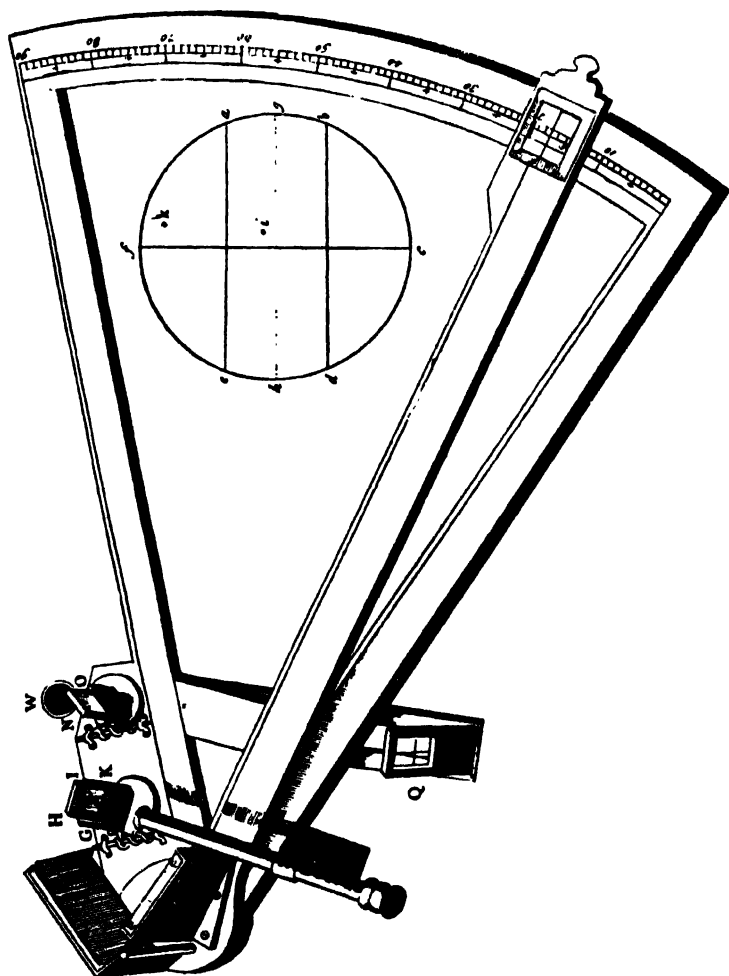
Hadley's Second Sea-Octant

instruments the observer had simultaneously to bring two different lines of sight into alignment with two different objects, while, with the octant, he had merely to judge of the coincidence of two images in the same line of sight.

Trials were made of Hadley's instrument in 1732 on board an

Admiralty yacht off Sheerness, in which Hadley himself took part, together with his brother Henry Hadley, and James Bradley. The results were encouraging (see *Phil. Trans.*, Vol. XXXVII, p. 341), but little improvement in the design of the instrument was made until after Hadley's patent had run out. Hadley's "sea-octant," as it came to be called, was employed to measure lunar distances (i.e., the angular separations of stars from the nearest or furthest point of the Moon's limb, according to which is illuminated), about 1747, by Captain John Campbell, who often observed with Bradley. It was Campbell who, in 1757, proposed to enlarge the octant into a sextant, so as to measure angles up to 120°—an improvement which gave the instrument practically its present form.

GODFREY

The claim of Thomas Godfrey to have invented an instrument for taking altitudes by means of a double reflection was brought before the Royal Society, in January 1734, in a letter from the American mechanician himself. Godfrey therein appealed to the contents of another letter, previously addressed to Halley, on his behalf, by his friend James Logan. (The letter is reproduced in Miller's *Retrospect of the Nineteenth Century*, Vol. I, p. 468.) Logan's letter, which was dated from Philadelphia, May 25, 1732, was produced to the Society; it was supported later by an affidavit made by one of Godfrey's sailor friends to the effect that Godfrey had described the invention to him towards the end of October 1730. On the other hand, Hadley had the priority, not only in the *publication* (as a comparison of the date of his paper with that of Logan's letter shows), but also in the *construction* of his invention, for the Society's Journal records that, on February 7, 1734, George Hadley exhibited a model of the instrument, which he said that he had made under instructions from his brother John about midsummer of 1730. Godfrey's reflecting instrument resembles, in all essential respects the *first* form of Hadley's as described above, the horizon-glass having an oblong unsilvered spot through which distant objects could be viewed directly. Godfrey had recognized the need for shades in order to reduce the glare of the Sun's rays, and he had even hit upon Hadley's device of reckoning the angular divisions on the limb at double their true value, as is still the practice.

There seems no reason to suppose, however, that Hadley or Godfrey derived any assistance from each other's inventions. This seems to have been the conclusion reached by the Royal Society at the time, though later on a popular legend arose to the effect that Hadley had seen Godfrey's instrument while serving as a naval officer in the West Indies. S. P. Rigaud established that there was

Hadley
(By courtesy of the Science Museum, London)

Harrison
(By courtesy of Lt.-Com. R. T. Gould)

Thomas Earnshaw

Ferdinand Berthoud

no officer of the name of Hadley in the British Navy between 1719 and 1743, and that throughout the critical months John Hadley was regularly attending meetings of the Royal Society.

(See a series of articles by S. P. Rigaud on the history of "Hadley's Quadrant" in the *Nautical Magazine*, 1832–34, Vols. I–III.)

B. The Marine Chronometer

To determine a ship's position at sea it is necessary to ascertain its longitude and latitude by independent astronomical observations. The latitude can be deduced from an observation of the meridian altitude of a celestial body whose declination at the time is known. In order, however, to determine the longitude, measured from some prime meridian, it is necessary to ascertain the local time at some instant and to compare it with the standard time corresponding to that prime meridian. The difference between the local and standard times then measures the required longitude from the prime meridian. The local time can be ascertained comparatively easily by appropriate observations with the sextant; but the problem of fixing the corresponding standard time presented great difficulties down to the middle of the eighteenth century. Although several theoretically sound methods were proposed, the data necessary for their application were not known with sufficient precision.

HUYGENS

Huygens suggested that several pendulum-clocks, set to show standard time, should be taken on the voyage; but this scheme would not work because, even if the clocks had kept time sufficiently accurately on shore (which they did not), the motion of the ship would immediately have put them out of order. Huygens, indeed, had rather more success with a type of clock which he designed especially for marine purposes about 1659. But even this instrument can scarcely have worked satisfactorily except in a dead calm.

Money prizes were accordingly offered by the governments of the chief maritime nations of the seventeenth and eighteenth centuries as inducements to inventors to contrive some means of solving the problem of longitude. In 1714 the British Government offered £20,000 for any practicable method of finding longitude within half a degree, with lesser prizes for less accurate methods. A Board of Longitude was set up to administer the funds, and before it was wound up in 1828 it had paid out more than £100,000 in assistance and rewards to inventors. Numerous claims to the rewards were made, but most of them were based on schemes of little value.

HARRISON

The problem of constructing a marine chronometer of the required precision was first solved by John Harrison (1693–1776), a Yorkshire-man who early showed genius in the construction and improvement of clocks. Among his inventions was the well-known "grid-iron" pendulum which bears his name, and the compensation curb, which played an important part in the mechanism of his chronometer, both depending upon the unequal thermal expansion of two metals.

In the simplest type of clock-pendulum, consisting of a metal rod with a bob at the free end, changes of temperature produce variations in the length of the pendulum, and concomitant variations in its period of vibration, the clock tending to go slower in summer than in winter. Harrison sought to eliminate this effect by using a pendulum made up of rods of two or more metals having unequal coefficients of expansion and so disposed that the effective length of the pendulum (the distance between the axis of suspension and the centre of oscillation) should remain constant throughout a considerable range of temperature. With this device may be compared George Graham's mercurial pendulum, invented about the same period: here the bob consists of a vessel containing mercury, the expansion and consequent rise in the centre of gravity of this metal just compensating for the downward expansion of the supporting steel pendulum rod, so as to keep the rate of vibration unaffected by moderate changes of temperature. Such devices are now generally superseded by the use of rods of "invar"—a nickel-steel alloy of negligible thermal expansion.

Harrison early resolved to compete for the prize offered by the Board of Longitude. With this object, in 1728, he submitted plans for a marine clock to George Graham, the instrument-maker, who privately lent him the money necessary for constructing the projected instrument. It was completed in 1735, and worked on a principle which provided an automatic compensation for the effects of the motion of the ship.

Harrison's first chronometer, which is now preserved at Greenwich Observatory, worked somewhat like a clock, in which, however, the place of the pendulum was taken by two massive balances (seen at the back of the machine). These were controlled by four balance springs, and always moved in opposite directions, so as to be equally and oppositely affected by the rolling of the ship. The machine weighed 72 lbs., and it showed days, hours, minutes, and seconds (see Illustr. 80). The driving power was supplied by two mainsprings; and there was a special contrivance to ensure that the instrument should not stop when being wound, as formerly occurred even in

clocks intended for astronomical purposes. Harrison expended much ingenuity in devising means of reducing friction in the working of his machine, and, for the first time, compensation was provided for the variations of the resistance of the balance springs with varying temperature. This was done by connecting the fixed ends of the springs to a manifold of brass and steel rods whose expansions and contractions (which were here *cumulative*) moved these ends so as automatically to regulate the tensions of the springs.

Harrison was sent with his instrument on a voyage to Lisbon and back, with encouraging results. Having obtained assistance from the Board he proceeded to build a second time-keeper (which, however, could not be tested at sea owing to a war with Spain), and afterwards a third.

Harrison's second chronometer (1739) did not differ essentially from his first, though a few small improvements were introduced. The third chronometer (1757) had circular balances, and compensation for temperature changes was obtained by fixing the effective end of each balance spring by means of curb pins attached to the free end of a "curb" consisting of a strip of brass and a strip of steel riveted together. The unequal thermal expansions of these two metals caused the compound strip to curl round by various amounts as the temperature varied, and thus,

Illustr. 79 —Harrison's Grid-Iron Pendulum

by moving the curb pins along the spring, to alter the effective length of the spring by pre-arranged amounts. These machines were of massive construction, the second of them weighing over a hundred

pounds. A fourth and much smaller instrument, however, resembling a large watch, was completed in 1759, and proved to be the finest of the whole series (see Illustr. 81).

Harrison's fourth chronometer measured about 5 inches across, and had an hour hand, a minute hand, and a seconds hand, all traversing the same enamel dial. It was controlled by a circular steel balance with three arms and a compensation curb of the type already described. The escapement was based upon the verge escapements employed in the watches of the time, but with many improvements tending to leave the balance much greater freedom of motion. As usual in Harrison's instruments there was a "maintaining power" to keep the chronometer going while it was being wound. Unlike the earlier chronometers of Harrison, this fourth one was not mounted on gimbals, but lay upon a cushion in a box during voyages. Under the care of Harrison's son William, it was tested on a voyage to the West Indies, during which it lost only five seconds. (The error of five seconds was *additional* to the error to be expected from the instrument's previously determined losing rate of 2⅔ seconds a day.) It was tried again on a voyage to Barbados in 1764, and qualified for the maximum award, but this the Board would not pay in full until Harrison had explained its construction and had shown that other clockmakers could construct similar and equally trustworthy instruments. A complicated quarrel ensued between the Board and Harrison, in which the King, George III, took Harrison's part in an appeal to Parliament; but before his death he had received the balance of the £20,000 still owing to him. Harrison's chronometers are to-day among the treasures of Greenwich Observatory.

Harrison's methods were developed after his death by Larcum Kendall (1721–95) and Thomas Mudge (1715–94). Mudge was the inventor of the lever escapement, now almost universally employed in watches and in chronometers intended to stand especially rough usage. But the later development of the marine chronometer owes most to the labours of two rival continental horologists of the eighteenth century, Le Roy and Berthoud, while the problem of producing numerous cheap instruments which could be widely used in navigation was solved by the English clockmakers, John Arnold (1736–99) and Thomas Earnshaw (1749–1829).

LE ROY

Pierre Le Roy (1717-85), a Frenchman, inherited his father's office of *Horloger du Roi*; and in 1748 he described his invention of an improved form of escapement, designed to obviate as far as possible any interference of the escape-wheel with the free motion
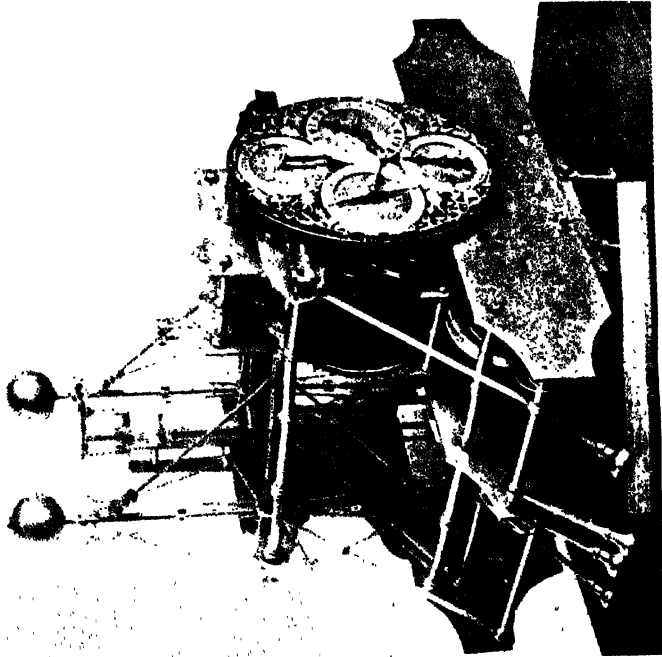
Harrison's Chronometer No. 1

Harrison's Chronometer No. 4

(By courtesy of Lt.-Com. R. T. Gould)
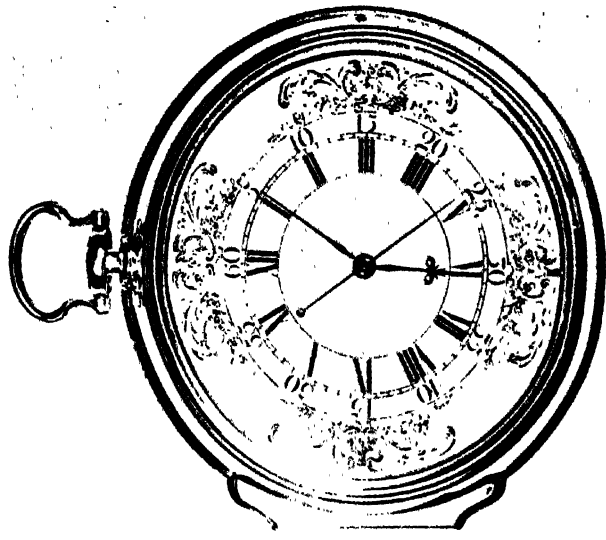
of the balance by which the timepiece is regulated, and to restrict the necessary interaction, which gives the balance its periodic impulse, to that part of its vibration where its natural motion is least liable to be affected. In 1754 Le Roy described his first somewhat crude design for a chronometer; its balance was a massive metal sphere turning upon a diametral axle against the torsional resistance of a straight spring from which the axle was suspended. No satisfactory compensation for changes of temperature would have been practicable with this instrument. After several further attempts, Le Roy, in 1766, succeeded in constructing a chronometer which may be regarded as the prototype of the modern instrument, which was destined to supersede Harrison's. Le Roy's was a remarkable invention for its originality and independence of current ideas. He described his instrument, and the logical process by which he had arrived at the conception of it, in his *Mémoire sur la meilleure manière de mesurer le temps en mer* (Appendix to Cassini's *Voyage fait par ordre du Roi en* 1768, *pour éprouver les montres marines inventées par M. le Roy*, Paris, 1770). The instrument embodied Le Roy's inventions of the compensation balance (he devised both the mercurial and the bi-metallic types), and of the detached chronometer escapement. The movement was regulated by a circular balance, and the effect of changes of temperature upon the balance springs was compensated by including in the balance system two thermometers filled partly with mercury and partly with alcohol. Any thermal expansion of the mercury displaced its centre of gravity towards the axis of the balance, and the resulting diminution in the moment of inertia of the system could be adjusted so as to neutralize the effect of the increase in moment of inertia consequent upon the expansion of the balance, as well as the effect of the weakening of the balance springs with rise of temperature. The curvature of the tubes could, in theory, be adjusted to give perfect compensation. As an alternative, Le Roy invented the more familiar bi-metallic balance; here the rim of the balance is made of strips of two metals, having unequal thermal expansions, riveted together, with the less expansive inside. The circumference is dissected into segments, each having one end fixed to a spoke of the balance wheel, and the other end loaded, and free to curl in towards the centre under the influence of the differential expansion, when the temperature is raised; this tends to compensate for the weakening of the balance spring, and the increase in moment of inertia due to the expansion of the spokes. Trials at sea established the excellence of Le Roy's chronometer, and of another of similar construction which he made subsequently. The original instrument, and the memoir in which it was described, received a double prize from the Academy.

BERTHOUD

Ferdinand Berthoud (1729-1807), a Swiss horologist who spent most of his life in Paris, showed much resource as an inventor and much industry as a writer on his art. In contrast to his great rival Le Roy, who solved his problems by submitting them to acute and original analysis, Berthoud learned most from experience, his own and other people's. The chronometers which he constructed show great diversities in design, in respect both of driving power and of regulation. Entering the field in 1763 with a crude marine clock which embodied most of the defects of the early chronometers, Berthoud gradually progressed, benefiting by the inventions of Harrison and of Le Roy, until he was producing instruments approximating in their design to those now in use. Berthoud is one of several horologists for whom the honour of the invention of the so-called "spring detent" escapement has been claimed.

ARNOLD AND EARNSHAW

The pioneers, Harrison, Kendall, and Mudge, even when they had standardized the designs of their chronometers, still required two or three years to construct a single instrument; and even Berthoud could not produce more than two or three of his machines in a year. Arnold and Earnshaw, however, increased the speed of production by a wise division of labour: having arrived at a satisfactory type of chronometer, they assigned the manufacture of its several parts to workmen specialists, and themselves only put the final touches to the instruments. Arnold excelled in the manufacture of pocket chronometers, and, though rather given to exaggerating his own achievements, he must be credited with several valuable innovations, including the "pivoted detent" and "spring detent" escapements and several new types of compensation balance; and he patented the spiral balance spring, though this had been anticipated by Harrison and others. The escapement and compensation balance now employed in chronometers was introduced by Earnshaw. His escapement, patented in 1783, is of the "spring detent" type (though an improvement on Arnold's); in it the locking stone, whose function it is to keep the escape wheel at rest when it is not impelling the balance, is restored to its position between impulses by means of a spring. His compensation balance consists of two brass and steel strips; each forms nearly half a circle, and is loaded at one end and joined at the other end to a cross-bar forming the diameter of the complete circle. Unlike his predecessors, Earnshaw cut the whole balance out of a disc of steel round which molten brass had been poured; no soldering, bending, or screwing of parts was therefore

required. The trial of Earnshaw's chronometers by the Board of Longitude, and his claims for reward, were the occasion of much bitter controversy between him and the supporters of his rival Arnold, as to the originality and priority of the inventions which Earnshaw claimed, apparently with justice.

SUBSEQUENT DEVELOPMENTS

In the nineteenth century the use of chronometers at sea rapidly extended, especially, at first, where scientific exploration, or navigation in little known regions, was concerned. Naval vessels were supplied with chronometers from about 1825. Much effort has been directed to improving the chronometer during the past 150 years. Progress has been made in matters of technical detail, and in the choice and finish of the materials employed. For example, the balance springs of superior chronometers are now generally made of palladium; even glass has been shown to have its good points for this purpose; and large claims are made for a nickel-steel alloy, "elinvar," whose elasticity is very little affected by changes of temperature. The performance of these instruments has also vastly improved. But despite a multiplicity of experiments in the design of the escapement, the balance, and the balance spring, Earnshaw's form of the instrument has proved, in all essentials, to be the best so far obtainable.

The essential feature of a good chronometer is not so much that its rate of gaining or losing should be *small* as that it should be *uniform* over long periods, so that the correct time can be obtained from the chronometer on a voyage by applying a definite known correction for the accumulated error. The *rating* of chronometers (i.e., the determination of the amounts which they gain or lose in twenty-four hours) has been an important part of the work of Greenwich Observatory since 1766. The use of the chronometer has superseded the method of lunar distances as a means of determining longitudes, and since 1907 the tables necessary for the application of that method have been omitted from the *Nautical Almanac*. A new factor has now been introduced into the problem of determining longitudes with the broadcasting of radio time-signals, whereby the standard time corresponding to a prime meridian is regularly transmitted to ships at sea for comparison with the local time ascertained by observations made on board. Since, however, this local time cannot in general be compared directly with the time-signals, but must be obtained when the observing conditions are suitable, the use of chronometers at sea is by no means superseded by the introduction of time-signals; these, in fact, simply afford a means of regularly

checking the indications of chronometers instead of trusting entirely to their continued uniformity of rate.

(The history of the marine chronometer is dealt with fully in Lt.-Commander R. T. Gould's *The Marine Chronometer, its history and development*, 1923, from which much of the above information and some of the illustrations are derived.)

PHYSICS

I. LIGHT   II. SOUND

I. LIGHT

LITTLE progress was made with the development of theories of light during the eighteenth century. The majority of physicists accepted some form of corpuscular hypothesis (or the "emission" theory, as it is frequently called), supposing the authority of Newton to be wholly on their side and ignoring the part played by aetheric waves in his explanations of periodic light phenomena. Newton had introduced such waves principally in order to account for the "fits" of easy reflection and easy transmission which were supposed alternately to possess the corpuscles. Many eighteenth-century physicists, however, were disposed to abandon the hypothesis of "fits" altogether. For instance, Boscovich (1758) accounted for the partial reflection and partial transmission of light, incident upon the surface of separation of two transparent media, by reference to a supposed polarity of the corpuscles, already suggested by Newton in connection with his explanation of double refraction. It was supposed that each light-corpuscle was endowed with different properties on different sides, e.g., with two poles, one of which was attracted and one repelled by matter, and that it was in a state of rotation whereby it presented its different sides alternately to the reflecting surface.

Those who adhered to the corpuscular hypothesis were anxious to see it established securely by direct experimental evidence, and a number of attempts were made to supply this. (See J. Priestley: *The History and Present State of Discoveries relating to Vision, Light, and Colours*, 1772, pp. 385-90.)

It was argued that if light consisted of material particles in rapid motion, it should possess a certain *momentum* which might be observed and measured. In 1708 Homberg reported to the French Academy of Sciences that he had detected a positive pressure of light; but doubt was cast on his results by Mairan, who attributed the results observed to convectional air-currents. Mairan himself could get no definite results even when he focussed light with a large lens upon a compass-needle or upon vaned wheels which turned under the slightest impulse. Under certain conditions such a wheel was indeed set in motion, but Mairan concluded that this was due to the heating of the air. John Michell told Priestley that he

had sought the effect with an instrument consisting of a fine wire having fastened to it at one end a very thin plate of copper, at the other end a counterpoise, and in the middle an agate cup and a short horizontal magnetized needle. The whole was mounted on a needle point, and placed in a box with a lid and front of glass. The wire was set at right angles to the direction of the Sun by means of an external magnet, and light was then thrown upon the copper plate by a two-foot concave mirror. The plate receded before the light until it struck the back of the box. The same thing occurred when the wire was reversed upon its bearing. This effect was attributed to genuine light-pressure, and from the numerical data of the experiment Priestley calculated the quantity of matter incident in the form of light upon one square foot in one second. He showed that at this rate the Sun would lose in weight just over two grains a day, which, he computed, would have shortened its radius only by about ten feet since the Creation, assuming the Sun to have the same density as water.

It is now known that light actually does exert pressure upon any material surface upon which it falls. This effect was detected experimentally by Lebedew in 1900, but it was probably not detected by any of the eighteenth-century experimenters, as it is very minute and is masked by convection and radiometer effects unless special precautions are taken. Since, however, such a pressure is to be expected on either the corpuscular or the electromagnetic theory, its significance as a means of discriminating between the rival theories has vanished.

The seventeenth-century theories of refraction and dispersion had suggested the question whether differently coloured rays of light might not travel, even in empty space, with different velocities. On this hypothesis, colour effects should be observed at the onset and at the conclusion of an eclipse of one of Jupiter's moons. Supposing the red rays to travel with the greatest speed, and the violet with the least, of the spectral colours, the satellite, for half a minute before being totally eclipsed by the planet, should show a succession of colours, beginning with white and ending with violet, while upon emerging again, it should first of all appear red. Newton had asked Flamsteed to look out for such colour effects, but they did not present themselves. The question was reopened, however, in the middle of the eighteenth century, by a young Scottish physicist, Thomas Melvill (1726–53)—one of the pioneers of spectrum analysis —and by the French optician De Courtivron. Search was made for the colour effects which they predicted in Jupiter's satellites by the astronomer James Short, but with a negative result. (See papers in the *Phil. Trans.*, 1753, 1754.)

Arago later improved on this method by examining the shadows cast by Jupiter's satellites upon the surface of the planet, to see whether their borders were coloured, as the hypothesis would require. He also applied the far more delicate test of observing the eclipsing variable star Algol to see if its periodic eclipses occurred simultaneously for all the colours. In neither case was any positive result obtained, showing that no appreciable difference exists between the velocities of propagation *in vacuo* of differently coloured rays.

EULER

The most eminent of those who advocated a wave-theory of light during the eighteenth century was Leonhard Euler, the mathematician. He marshalled the arguments against the corpuscular theory, and explained his own ideas in his popular *Lettres à une Princesse d'Allemagne* (written between 1760 and 1762), while he made more solid contributions to the subject in memoirs contributed to the Berlin Academy.

Euler argued that if the Sun were constantly sending out floods of particles moving at enormous speeds in all directions, its substance would soon be consumed, or it would at least in course of time show some noticeable diminution in size. Moreover, the light-corpuscles coming from the Sun would encounter and interfere with those coming from the stars and from other luminous bodies, whose outlines would therefore appear indistinct; but we perceive no trace of such interference. If space were filled with light-corpuscles, these would act like an aether in resisting the motions of the planets, and the special advantage of the corpuscular hypothesis would therefore be lost. Again, on the corpuscular theory, transparent bodies must be regarded as honeycombed with rectilinear pores extending from each point in every direction; yet such bodies often appear very solid.

Attempts to meet Euler's objections were made by advocates of the corpuscular theory whose arguments are summed up in Joseph Priestley's *History and Present State of Discoveries relating to Vision, Light, and Colours* (1772, pp. 359 ff.). It was maintained that the particles of light were incomparably small in relation to the distance separating neighbouring particles, even where their distribution was most dense. Hence beams might cross one another without mutual interference. Again, the momenta of the particles were too small for them to affect the motions of the planets. To explain how light could penetrate solid bodies, adherents of the corpuscular theory invoked the hypothesis of Boscovich, who taught that matter was not continuous, but was made up of physical points surrounded by

spheres of attraction or repulsion. They further pointed out that the corpuscular hypothesis simplified the task of explaining the phenomena of astronomical aberration and of phosphorescence, both of which excited much interest in the eighteenth century. The phenomenon of phosphorescence, it was thought by some, seemed to favour the corpuscular hypothesis, inasmuch as the nature of phosphorus, or "Bolognian stone," seemed to be readily explained on the assumption that the substance imbibed particles of light, retained them for some time, and emitted them on occasion, especially when heat was applied. (See, e.g., Priestley, *op. cit.*, pp. 360 f.)

In the positive contributions which he made to the theory of light, Euler, like Huygens, started from the assumption that the space between the heavenly bodies is filled with an extremely fine material aether, which also plays an important part in Euler's explanations of gravity, magnetism, and electricity. This is a fluid, like the air, but, according to Euler, a thousand times as elastic as air, and incomparably more finely divided, since the heavenly bodies traverse it without encountering any sensible resistance. Moreover, the aether has the property of spreading out in all directions and filling up all empty spaces. Hence it must not only exist in the heavens but must penetrate the atmosphere, and press into the interstices of all terrestrial bodies. Since the air, in consequence of its similar properties, is adapted for taking up the vibrations of sounding bodies and propagating them in all directions, thus giving rise to *sound*, it is natural to suppose that the aether, under similar conditions, will take up regular impulses and will convey them as waves in all directions, and to a much greater distance than sound will travel. Such agitations of the aether constitute *light*, whose enormous velocity follows from the low density and high elasticity of the aether. Thus nothing material actually comes to us from the Sun, any more than from a bell whose sound strikes our ear. There is therefore no reason to fear that the Sun, in shedding light, will suffer the slightest loss of substance. Terrestrial bodies, it is true, are consumed in the process of giving out light, but Euler correctly explained that this is due to their giving out smoke and vapours as well. The production of light alone, he held, involved no loss of substance, as could be proved by shaking mercury in an evacuated tube, no loss in weight accompanying the resulting phosphorescence.

Euler supplemented Huygens' theory of light with the doctrine that colour is determined by the frequency of the corresponding aether-vibrations, and is thus analogous to pitch in sound. He doubted, however, whether it would ever be possible to estimate the frequencies of the aetheric vibrations. Sunlight appears white

because it consists of vibrations of every frequency. Upon refraction it is resolved into waves of various wave-lengths; these, after separation, give rise to the simple colours. Euler likened the colours of the spectrum to the notes of the octave, and he supposed, on this analogy, that beyond the violet one would pass through purple to a second red whose frequency would be twice that of the ordinary red. He adduced as evidence for this view the periodic recurrence of the same succession of colours in a thin film of gradually increasing thickness.

Euler attributed the visibility of non-luminous bodies, not to the reflection of light, as Newton had done, but to a sort of resonance effect. The particles composing the surface of such a body, he supposed, are at rest, or are vibrating less violently than those of a luminous body, and hence they do not emit light on their own account. When, however, light falls on such a body, its surface-particles are set in sufficiently violent vibration to send out rays which give us an image of the body. Euler thus likened these particles to stretched strings having each its characteristic period of vibration. Just as such strings can be set in sympathetic vibration by notes corresponding to their respective fundamentals, so the particles of a body may be supposed to behave in relation to the vibrations of the aether. A body appears to us red when its particles vibrate sympathetically with a definite frequency corresponding to red light. The body appears white when its particles, according to their various states of tension, are attuned to all the vibrations which sunlight contains. It appears black if its particles are too heavy to take up any vibration at all. When the illumination is cut off, the sympathetic vibration ceases and the non-luminous body becomes invisible, except for certain phosphorescent substances, with which Euler was acquainted.

Euler explained the coloration of thin transparent films by a supposed analogy with the production of notes by the vibration of air in organ-pipes. The aether in the film is capable of vibrating in a certain period depending only upon the thickness of the film. Incident light whose colour corresponds to this period sets the aether in vibration, and light of that colour is re-emitted, i.e., reflection of that colour is observed. If, however, the periods do not agree, the incident light passes through the plate and produces no vibration in the contained aether, and the corresponding colour is absent from the reflected light. This analogy led Euler correctly to assign the shortest period of vibration to blue light, and the longest to red light; but later a different analogy led him to reverse this correlation.

From this account of Euler's ideas it will be clear that he came

very near to the conception of the occurrence of colours which later developed out of the undulatory theory. Despite the clarity with which he expounded his views on the nature of light, however, Euler introduced no fresh experimental evidence, and the corpuscular theory remained unshaken for the time being. Nevertheless, he started a line of enquiry which led to the correction of an inaccurate law formulated by Newton, whose authority in optics was thereby weakened.

Newton had been led by some experiments to suppose that the dispersion produced by a lens bore the same constant proportion to the deviation of the rays whatever the medium, so that it would be impossible to eliminate chromatic aberration from a lens-system without neutralizing the refracting effect. His influence induced many astronomers to turn from refracting telescopes to reflecting ones. Euler, however, writing in 1747 (*Sur la perfection des verres*

A͟C B

A̅C̅B

Illustr. 82.—Euler's Achromatic Lens-Combination
AA, BB are glass lenses, CC is water.

objectifs des lunettes, Hist. de l'Acad. Roy. des Sciences, Berlin), stated (wrongly) that the human eye, at least, was so constructed as to be free from chromatic aberration—a point already made by David Gregory (1695). He concluded that this defect could be remedied in an artificial lens-system by a suitable combination of several different transparent media, as in the eye. He worked out the general case of refraction through five successive media, assuming a dispersion-law of his own, which he held was experimentally indistinguishable from that of Newton. He then applied his result to the case of a lens-combination constructed by enclosing water between two con-cavo-convex glass lenses (Illustr. 82), cementing these together at the edges. He discovered the relation which must subsist between the radii of curvature of the several surfaces in order that the red and violet rays should come to the same focus. Practical difficulties arose, however, when attempts were made to put Euler's suggestion into practice. It was found that, though the chromatic effects were mainly eliminated, the lenses, having large curvatures, gave rise to considerable spherical aberration.

John Dollond (1706–61), a London optician, criticized Euler's paper. A critical examination of Newton's law of dispersion by the Swede, Samuel Klingenstjerna, however, prompted Dollond to repeat the experiments underlying that law, with the result that he was led to abandon it. He found that it was possible, by passing rays through prisms of water and glass in succession, to neutralize the refraction without completely neutralizing the dispersion, and vice versa (*Phil. Trans.*, 1758). He experimented with glass-and-water lens-combinations, but eventually obtained the best results when using combined flint-glass and crown-glass lenses, by which the chromatic aberration, though never in theory absolutely eliminated, was reduced to negligible proportions; and so the refracting telescope was reinstated in the good opinion of astronomers.

After Dollond's death it was found that an achromatic telescope had been constructed as early as 1733 by one Chester More Hall, a country gentleman of Essex, who had had composite lenses constructed to his specifications, but had not made his invention known.

PHOTOMETRY

BOUGUER

The eighteenth century saw the establishment of precise methods of photometry. Kepler had arrived intuitively at the fundamental law that the intensity of light varies inversely as the square of the distance from the source; Huygens had first experimentally compared the intensities of various luminous bodies, and some pioneer work had been done by Buffon. The first effective photometer, however, was constructed by the French physicist, Pierre Bouguer (1698-1758), Condamine's colleague in his geodetic expedition to Peru.

This instrument consisted of two transparent screens placed over openings O, O¹, in two opaque screens EC, CD (Illustr. 83). Each opening was illuminated by one of the two sources to be compared.

Illustr. 83.—Bouguer's Photometer

Between these a partition F was placed, in order that each source should produce its separate effect. The distances AO, BO¹ were varied until the screens at O and O¹ appeared equally bright to an eye situated in front of them. The intensity of each source was then known to be proportional to the square of its distance from the screen which it illuminated. Bouguer devised several other types of

photometer, all based on the same general principle. With these instruments he was able to show how the absorption of light accompanying its reflection varies according to the angle of incidence and to the nature of the reflecting surface, and how the absorption suffered by light in passing through a layer of a transparent substance depends upon the thickness of the layer. He showed also how the absorption of a star's light varies with its altitude above the horizon; and he compared the apparent brightness of the Sun and Moon.

LAMBERT

Bouguer's *Traité d'Optique sur la gradation de La Lumière* was first published in 1729. In 1760 there appeared another fundamental work on the same subject. This was the treatise of the distinguished German physicist and philosopher, Johann Heinrich Lambert (1728–77), entitled *Photometria, sive de mensura et gradibus luminis, colorum et umbrae*, Augsburg, 1760. (An annotated German translation of the work is contained in Vols. XXXI–XXXIII of Ostwald's *Klassiker.*)

Lambert's researches in photometry were so exhaustive that since the appearance of his great work on this subject very few photometrical problems have been raised and discussed which he had not already treated or glanced at. In the arrangement of ingenious and careful experiments Bouguer excelled Lambert, who in his experimental investigations even misleads owing to a certain negligence. Lambert's apparatus consisted only of three small mirrors, two lenses, several glass plates, and a prism. On the other hand, to Lambert is due the credit of creating the concepts and system of photometry. While Bouguer restricted himself to observations, and only drew from these the more obvious inferences, Lambert knew how to give a complete solution to each problem. Occasionally, it is true, this was possible only by such a far-reaching simplification of the assumed conditions that the result of the calculation could be regarded only as a rough approximation to the actual circumstances.

Lambert's *Photometria* is divided into seven parts dealing respectively with (1) first principles and the properties of direct light; (2) light passing through transparent media, and the intensities of lens-images, caustics, etc; (3) light reflected from opaque surfaces, polished or rough; (4) physiological optics, e.g., the relation between the apparent brightness of an object and the aperture of the pupil of the eye; (5) the scattering of light which passes through transparent media (such as the atmosphere), twilight, etc.; (6) the comparative luminosities of the heavenly bodies corresponding to their various distances and phases; (7) the relative intensities of coloured lights and of shadows.

Lambert begins by considering the fundamental ideas of photo-metry. He holds that it is just what our senses are continually encountering that most eludes our insight. The theory of light affords an excellent example of this. Its insufficiency is shown by the fact that two such different hypotheses as those of Newton and Euler (or rather Huygens) are employed for the explanation of the same phenomena. The former hypothesis is the more easily under-stood, but Euler's theory corresponds better to the nature of things. Lambert adds the often repeated maxim concerning the judgment of hypotheses: "Among the most important and trustworthy criteria that an hypothesis approximates to the truth, must be counted the case when it is possible, by means of the hypothetical theory, to anticipate the occurrence of new phenomena, and when it is possible to deduce from it propositions with which experiments, arranged for the purpose, are in agreement." Such a test was destined later to decide in favour of the wave-theory of Huygens and Euler.

As there was no absolute measure for photometric investigations, such as a thermometer provided in the study of heat, so that a highly subjective factor, the judgment of the eye, had always to be taken into account, Lambert made the assumption that a light stimulus "remains the same as long as the same eye is affected by it in the same manner." The eye, he supposed, was not able, in the presence of different degrees of brightness, to decide by how much one is greater than another; but it had to be assumed that the eye could judge whether two sources of light were equal or not. Only by combining this assumption with the principles of photometry already deduced from geometrical considerations could this branch of optics be developed.

Of such principles, Lambert lays especial emphasis upon two, in addition to the inverse square law of the intensity of light. The first of these runs: "If the same surface is illuminated at one time by m and at another time by n sources of light, each of which has the same intensity and sends its light to that surface in exactly similar circum-stances, then the respective degrees of brightness are to each other as m to n." The other fundamental law is to the effect that the brightness of the illumination of a surface falls off in the same proportion as the sine of the angle of inclination of the incident beam to the surface. Lambert's geometrical demonstration of this law is given in most text-books of physics. He was not content, however, with merely theoretical proofs of these propositions, but he sought by appropriate experiments to demonstrate their mutual interdependence and so to invest them with greater certainty.

The second chapter of Part I of the *Photometria* deals with the quantities of light emitted by surfaces of various shapes. The photo-

meter employed by Lambert bore a considerable resemblance to the instrument later called after Rumford. Lambert proceeded by comparing the illuminations of two surfaces, one of which was illuminated by a source of known intensity, and the other by the source whose intensity was to be ascertained. His apparatus is shown in Illustr. 84. The sources to be compared are at K and A; BDCEFG is a white, smooth wall before which, at HI, is placed an opaque screen. The source A casts a shadow of this screen which covers the portion DCEF of the wall, while the shadow cast
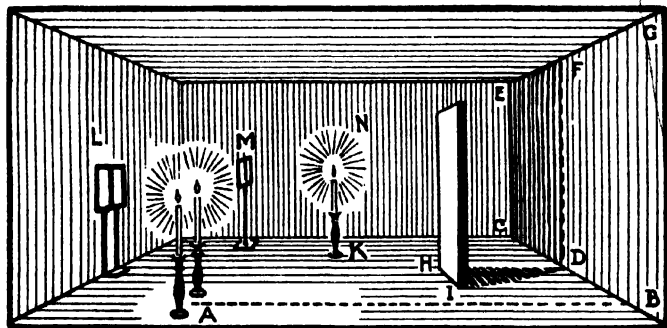
Illustr. 84.—Lambert's Photometer

by K falls on the portion BDFG. Thus the portion BDFG is illuminated only by the source A, while DCEF is illuminated only by K. One of the sources is then moved backwards and forwards until the wall appears equally bright on either side of the line DF, when simple measurement enables the relative intensities of the lights to be ascertained.

Among the numerical results obtained by Lambert may be noted the following: the intensity of light falling vertically on the Earth's surface is reduced by atmospheric absorption in the proportion 59: 100; and the ratio of the mean brightness of the full Moon to that of the Sun is as 1 : 277,000, while its ratio to the mean *central* brightness of the full Moon is 2: 3.

LIGHT AND HEAT. SPECTRUM ANALYSIS

Some contributions to the study of the relations between light and heat, and of spectrum analysis, were made by Thomas Melvill, the Scottish physicist whose name has already been mentioned earlier in this chapter. Priestley had pointed out (*op. cit.*, p. 373) that heat promotes the emission of light from phosphorescent substances

previously exposed to light. Melvill suggested that the heat produced in a body by incident light represented the *reaction* of the light-corpuscles on the body, corresponding to the *action* of the body upon the light-corpuscles involved in its reflecting or refracting the light. In this way he also tried to explain why sunlight passes through the atmosphere without heating it appreciably, namely because there is no appreciable reflection or refraction in the passage of light through the atmosphere. (See Melvill in *Edinburgh Essays and Observations, Physical and Literary*, Vol. II. 4.)

Melvill's pioneer experiments in spectrum analysis may be described very briefly in his own words. He mixed various salts with burning spirits, and "having placed a paste-board with a circular hole in it between my eye and the flame of the spirits, in order to diminish and circumscribe my object, I examined the constitution of these different lights with a prism (holding the refracting angle upwards). . . ." He noted which colour predominated in each case, and observed that in the case of sea-salt a bright yellow light (now known to be characteristic of sodium) was predominant in a striking manner, and formed in the prism a sharp image of the aperture through which the flame was viewed. "Because the hole appears thro' the prism quite circular and uniform in colour; the bright yellow . . . must be of one determined degree of refrangibility; and the transition from it to the fainter colour adjoining, not gradual, but immediate" (*loc. cit.*).

smith's "opticks"

Before concluding the present chapter reference must be made to one of the most noted textbooks of the eighteenth century, namely, a comprehensive textbook on Light, written by Robert Smith (1689–1768), Master of Trinity College, Cambridge, and founder of what are still known as "Smith's Prizes" in mathematics. His *Compleat System of Opticks in Four Books*, Cambridge, 1738, exerted much influence, and was translated into French and German. Of the four books into which the work is divided, the first deals in a non-technical manner with the fundamental experiments in optics, while the second provides a more formal treatment of the geometrical theory of the subject. Smith studied the problem of spherical aberration in greater generality than his predecessors, Barrow and Huygens. The third book describes apparatus for grinding and polishing lenses and specula, and it gives a complete account of the construction, adjustment, and use of the principal optical instruments, while the fourth book gives a history of telescopic discoveries in the heavens. Smith was also the author of an important textbook on *Harmonics* (1748).

(On Physics generally see F. Cajori, *History of Physics*, 2nd. ed., N.Y., 1929; J. C. Poggendorff, *Geschichte der Physik*, Leipzig, 1879; F. Rosenberger, *Geschichte der Physik*, Braunschweig, 1882–90; E. Gerland and F. Traumüller, *Geschichte der physikalischen Experimentier-kunst*, Leipzig, 1899; W. F. Magie, *A Source Book in Physics*, New York and London, 1935. On Light, see E. Mach, *The Principles of Physical Optics*, Tr. by Anderson and Young, 1926; E. T. Whittaker, *A History of the Theories of Aether and Electricity*, 1910; N. V. E. Norden-mark and J. Nordstrom, "Invention of Achromatic Lenses," *Lychnos*, 1938, 1939.)

II. SOUND

The eighteenth century witnessed considerable progress towards the establishment of acoustics as an exact science. The most important experimental work in this field was carried out by Sauveur and Chladni; and the leading mathematicians of the period also did their share. The acoustic problems investigated were sufficiently varied, including those of the nature of "beats" and new methods of determining the pitch of sounds, the propagation of sounds by means of membranes, rods, and various kinds of gas, and the limits of the audibility of sounds.

BEATS AND PITCH

It was known early in the eighteenth century that upon sounding together two deep organ-notes differing slightly in frequency, periodic variations in the intensity of the resulting tone were heard, such as are now called "beats." Joseph Sauveur (1653–1716) recognized that this effect was due to the periodic coincidence of the vibrations producing the two notes, or their agreement in phase, as we should now say. The frequency of such beats equals the difference of frequency of the constituent notes. Sauveur based upon this principle his method of determining the frequency of any given note. His procedure was to count the number of beats per second which it made with a near note whose frequency stood in a known ratio to that of the given note. Thus upon sounding together two organ-notes differing by a semitone, their frequencies standing in the proportion of 15 to 16, he counted six beats a second. Knowing the ratio and the difference of the frequencies, he was able to deduce their values as 90 and 96 vibrations a second. Having obtained the frequency of a standard note, Sauveur was able to work out the frequencies of the remaining notes of the scale. He found that an open organ-pipe about 5 feet long gave a note of frequency 100, which he proposed as a standard of pitch. He arrived at this result by finding the note given by a pipe when another pipe whose length was adjusted so as to stand to that of the first as 99 to 100, gave a note which made one beat per second with the note of the first, upon sounding both

simultaneously. Euler, in 1739, based a more precise method of defining pitch absolutely upon Brook Taylor's formula (*Phil. Trans.*, 1713) which in its modern form connects the frequency (n) of vibration of a string with its length (l), tension (T), and mass per unit length (m):

$$n = \frac{1}{2l}\sqrt{\frac{T}{m}}$$

(*Tentamen novae theoriae musicae*, 1739).

During the eighteenth century various suggestions were advanced to explain how vibrating bodies produce their characteristic notes and overtones. Thus certain physicists supposed that a sound originated from the vibrations of the ultimate particles of the sounding body. In support of this view, De la Hire pointed out in 1716 that while a pair of tongs produce a note when tapped, they fail to do so when the arms are allowed to vibrate as a whole (*Mém. de l'Acad.*, Paris, 1716). This view was also maintained by C. B. Funk in 1779 (*Dissertatio de sono et tono*, 1779). At the end of the eighteenth century Thomas Young referred to the reaction of the different parts of a vibrating string on each other as the cause of the notes and overtones (*Phil. Trans.*, 1800).

By the middle of the eighteenth century the partial discovery had been made of what are now known as *combination-tones*. The periodic beats studied by Sauveur are heard only when the notes simultaneously sounded are of nearly equal frequencies. If, however, the interval between the notes be gradually widened, the frequency of the beats (which is the difference of the frequencies of the notes) becomes too great for the individual beats to be distinguished. But in this case a tone becomes audible, the tone, like the beats, having as its frequency the difference of the frequencies of the original notes. Such tones, later called *grave harmonics*, were described by G. A. Sorge (*Vorgemach der musikalischen Komposition*) in 1740, and by F. Romieu (*Mém. de la Société Royale des Sciences*, Montpellier) in 1753; but they are often associated with the name of the Italian musician G. Tartini (whence they were also called *sons tartiniques*). Tartini did not describe this *terzo suono*, as he called it, until 1754 (*Trattato di musica . . .*), but he claimed to have noticed it as early as 1714. He was unable to account for the existence of this tone, which, moreover, he gave as an octave above its true pitch. Lagrange stated in a memoir of 1759 that the grave harmonic of Tartini was produced by the beats between two notes when the frequency of the beats became sufficiently great to correspond to a note of audible pitch. The periodic variation in the joint intensity of the two notes (which constitutes beats) was thus regarded as itself equivalent to a

note having the same period of vibration. This view of the matter is not now generally maintained. It has been superseded by Helmholtz' theory of combination-tones, which, though not free from difficulties, accounts for the existence of other supernumerary tones which are found to arise from the two fundamental ones. These combination-tones have frequencies which are the sums and differences of multiples of the frequencies of the fundamental tones. Sometimes they are produced by the conditions under which the sound is emitted, and sometimes by the conditions under which it is received at the ear.

The methods of the calculus were applied to the problem of the form and motion of a transversely vibrating string, chiefly by Brook Taylor, D'Alembert, Daniel Bernoulli, and Euler, their investigations being completed by the early work of Lagrange. Euler also considered the more complicated vibrations which are obtained by compounding several independent linear vibrations. The vibration of membranes was investigated by Riccati, who published his results in 1786. Related problems were presented by the vibration of solid bodies under their own elastic forces. The transverse vibration of rods, which forms the basis of the tuning-fork and of several musical instruments, such as the harmonicon and the musical-box, was investigated by Daniel Bernoulli, Riccati, and Euler. They considered the various cases which arise according as one or both ends of the rod are fixed or free. A special study of longitudinal and torsional vibrations in rods was made by their discoverer, Ernst Florens Friedrich Chladni (1756–1827). He described his results in his book *Über die Longitudinalschwingungen der Saiten und Stäbe* (Erfurt, 1796).

Illustr. 85.—Chladni's Sound Figures

Of especial interest were Chladni's pioneer investigations on the vibrations of plates, described in his *Neue Entdeckungen über die Theorie des Klanges*, Leipzig, 1787, and in his *Akustik* of 1802. About 1785 Chladni was led to examine the vibrations of discs, square plates, etc., made of glass or metal, which were clamped, usually at the centre, and were excited by means of a violin-bow applied to the edge. He noted relations between the frequencies of the various notes which a plate produced under these conditions. Later he

hit upon the idea of scattering sand upon horizontally clamped plates. Upon bowing the latter at the edge vertically, the sand shifted from the vibrating to the non-vibrating parts of the plates and so formed configurations, or sound figures, which still bear Chladni's name. The position of the sand showed the nodal lines, and revealed the mode of vibration of the plate. After seeing these experiments, Napoleon remarked that "Chladni made sounds visible." A vast variety of such figures was produced, which Chladni sought to describe and classify in his works. Some of Chladni's acoustical figures are shown here (Illustr. 85). The first diagram shows the configuration of the sand when the square plate is clamped horizontally at the centre and the violin-bow is applied vertically at one of the corners; in the second diagram the bow has been applied at the middle of one of the sides. The third diagram shows the sand configuration when the plate is clamped at n or g and the violin-bow is applied at a. The fourth diagram shows another configuration into which the third easily changes.

THE INTENSITY OF SOUND

The experiments of Guericke, Hauksbee, Boyle, and Papin, in the seventeenth century, had shown that the intensity or loudness of a sound (as distinguished from its pitch or height) varies with the density of the air in which it originates. This discovery appears to have suggested the possibility that the loudness of a sound might be different in different gases, and vary proportionately to their several densities. Joseph Priestley was the first to carry out experiments to ascertain this. Placing a bell successively in glass globes filled with different kinds of gas, he measured the distances at which the sound was still audible in the several cases. He found that in hydrogen the sound was almost as inaudible as in a vacuum; in oxygen the sound was stronger than in air; and in carbonic acid gas it was about 50 per cent stronger than in air. He concluded that the intensity of the propagation of sounds in gases is proportional to their density or specific weight (*Experiments and Observations*, 1779). Perolle, who repeated Priestley's experiments, obtained rather different results. Taking as his unit the intensity of the propagation of sounds in air, he obtained the following values for the several gases with which he experimented: hydrogen, 0·234; carbonic acid gas, 0·82; nitrous (or "laughing") gas, 1·23; oxygen, 1·135 (*Mém. de l'Acad. de Toulouse*, 1781).

William Derham (1657–1735) attempted to determine the influence of variations of temperature, of the direction of the wind, and of the moisture of the atmosphere on the intensity of sounds. But his

results were rather vague. In general he found that sounds are weaker in summer than in winter; that they are stronger and harsher when there are easterly or northerly winds than when there are westerly winds; and that the sound of firearms is not weakened in wet weather, but is sometimes only barely audible in fine dry weather (*Phil. Trans.*, 1708).

MEDIA AND VELOCITY OF SOUND

Chladni's investigations into the longitudinal vibrations in rods made of various substances led him to investigate also the various velocities of sound in different media. He could not measure these velocities directly (as Biot did subsequently) but only indirectly, namely by deducing the velocity from the rate of vibration, and the rate of vibration from the pitch of the note produced. Taking for his unit the velocity of sound in air, Chladni obtained the following comparative velocities for the various materials with which he experimented: tin, $7\frac{1}{2}$; iron, 17; silver, 9; copper, 12; glass, 17; wood, 11–17. He also turned his attention to the velocity of sound in different kinds of gas. Here, as in the case of the solid media, Chladni's measurement of velocity was indirect (direct measurements were made afterwards by Regnault). Using organ-pipes which were made to produce sounds in various gases, Chladni concluded from his experiments that the velocity of sound is greatest in hydrogen and slowest in carbonic acid gas (*Über die Töne einer Pfeife in verschiedenen Gasarten*, in *Voigt's Magazin der Naturkunde*, 1798).

LIMITS OF AUDIBILITY

Already in the eighteenth century attempts were made to assign limits within which the frequency of a series of sound-waves must lie in order to produce a musical note in the human ear. These limits of audibility of notes depend partly on the nature and intensity of the sound, and partly on the individual hearer, and estimates of them have always shown great variety. Sauveur concluded from his experiments with organ-pipes that the lower limit was $12\frac{1}{2}$ and the upper limit 6400 vibrations per second. Euler's final limits were 20 and 4000 respectively. Modern investigators place the lower limit at about 30; estimates of the upper limit vary greatly but average about 30,000 vibrations per second.

Illustr. 87

Chladni

Illustr. 86

John Dollond
(By courtesy of the Science Museum, London)

Illustr. 89

Black

Illustr. 88

Rumford
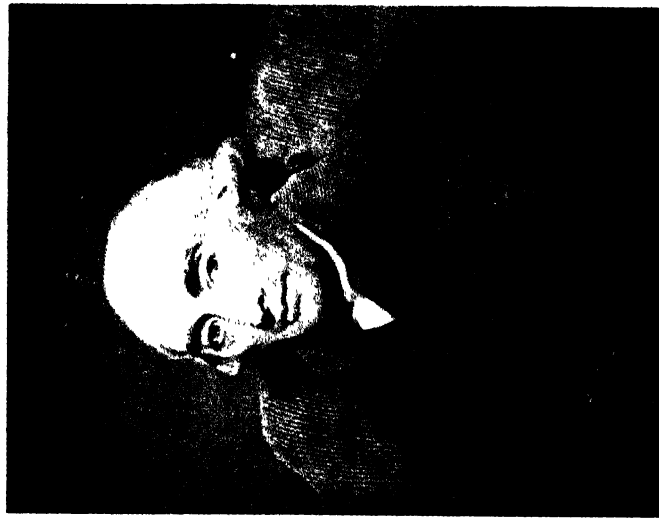
PHYSICS

III. HEAT

THE most notable feature of the scientific study of heat in the eighteenth century is to be found in its experimental work in calorimetry. Joseph Black took the lead in this kind of investigation, though his neglect to publish his work has prompted some historians to give the credit for it to the Swedish physicist, Johan Carl Wilcke (1732–96), whose work in this field of research was carried out somewhat later. Black's experimental work was further developed by Lavoisier and Laplace, who owed a great deal to Black's discoveries, though they appeared unwilling to acknowledge their indebtedness. The eighteenth century also witnessed the beginnings of exact measurements of thermal expansion, and the contributions made by Count Rumford to the study of the relation of heat to work, though his efforts had to wait for their crowning success until Joule carried out his researches in the nineteenth century.

A. THE CALORIC THEORY

The experimental work of the eighteenth century in connection with heat was largely independent of ultimate theories as regards the nature of heat. Some of the leading experimenters in this field affected a certain indifference to such theories, in which the seventeenth century was so deeply interested. This was the case, for instance, with Lavoisier and Laplace. Nevertheless such a theory was at the basis of the work done. And some of the workers, notably Black, were fully conscious of it. The theory in question may be described as the caloric theory. Caloric[1] was conceived as a kind of all-pervading, imponderable, highly elastic fluid the particles of which were attracted by matter and repelled by one another. When two bodies of different temperatures came into contact, it was supposed that caloric flowed from the hotter to the colder body until equilibrium was established in the two systems of material and caloric particles. When expansion resulted from heating, the expansion was attributed to the mutual repulsion of the caloric

[1] The term "caloric" was substituted for the older expression "matter of heat" in the *Méthode de Nomenclature Chimique* (Paris, 1787, pp. 30 f.) by De Morveau, Lavoisier, Berthollet, and Fourcroy.

particles which entered the expanding bodies when heated. The development of heat (by friction) when bodies were compressed was explained as due either to the fact that the particles of a body abraded by friction lost some of their capacity for caloric, which was thus liberated and made the body appear hot, or to the fact that friction and pressure squeezed out some of the caloric latent in the pressed body which thereby became sensibly hot. The caloric theory was perhaps natural in physics in an age in which phlogiston was accepted by chemists (see Chapter XIII). But it survived the phlogiston theory and dominated the science of heat until the middle of the nineteenth century. This was largely due to the valuable experimental work of Black. For an ingenious scientist, as the history of science has abundantly shown, any working hypothesis, even a false hypothesis, is better than none.

B. Thermal Capacity

Before Black's time, it was generally believed that the amount of heat required to raise the temperature of any body was proportional to the density of that body, or, in modern terms, that the thermal capacities of equal weights of all bodies were the same. About 1760, Black began to consider this opinion. He knew that Fahrenheit had mixed water and mercury at different temperatures and had found that the heating and cooling effect of mercury was only two-thirds that of the same volume of water, whereas the density of mercury was thirteen times that of water. Boerhaave had reported this result, from the consideration of which Black concluded that "the quantities of heat which different kinds of matter must receive to reduce them to equilibrium with one another, or to raise their temperatures by an equal number of degrees, are not in proportion to the quantity of matter in each, but in proportions widely different from this." Fahrenheit's experiment, then, led Black to the method of comparing the heating and cooling effects of other substances with the heating or cooling effects of an equal bulk of water. This method led to the modern system of *specific heats*. Black called the values thus obtained for different substances their "capacities for heat." Robinson has described Black's method as follows: "Dr. Black estimated the capacities, by mixing the two bodies in equal masses, but of different temperatures; and then stated their capacities as inversely proportional to the changes of temperature of each by the mixture. Thus, a pound of gold of the temperature 150°, being suddenly mixed with a pound of water of the temperature 50°, raises it to 55° nearly. Therefore the capacity of gold is to that of an equal weight of water as 5

to 95, or as 1 to 19; for the gold loses 95° and the water gains 5° (*Lectures*, I, p. 506).

The method of mixture followed to-day, where the masses are different and where many refinements of technique have been introduced, developed out of this method. In its essentials, calorimetry, as an exact scientific procedure, was introduced by Black; and under the name of "capacity for heat" the idea of "specific heat" is essentially due to him.

C. LATENT HEAT

BLACK

Black's best known discovery was that of "latent heat." He was led to it by the study of the problem of fluidity. He relates that in his time "Fluidity was universally considered as produced by a small addition to the quantity of heat which a body contains, when it is once heated up to its melting-point; and the return of such body to a solid state, as depending on a very small diminution of the quantity of its heat, after it is cooled to the same degree; that a solid body, when it is changed into a fluid, receives no greater addition to the heat within it than what is measured by the elevation of temperature indicated after fusion by the thermometer; and that, when the melted body is again made to congeal, by a diminution of its heat, it suffers no greater loss of heat than what is indicated also by the simple application to it of the same instrument.

"This was the universal opinion on this subject, so far as I know, when I began to read my lectures in the University of Glasgow, in the year 1757. But I soon found reason to object to it, as inconsistent with many remarkable facts, when attentively considered; and I endeavoured to show that these facts are convincing proofs that fluidity is produced by heat in a very different manner. . . .

"If we attend to the manner in which ice and snow melt when exposed to the air of a warm room or when a thaw succeeds to frost, we can easily perceive that however cold they might be at the first, they are soon heated up to their melting point, or begin soon at their surface to be changed into water. And if the common opinion had been well founded, if the complete change of them into water required only the further addition of a very small quantity of heat, the mass, though of a considerable size, ought all to be melted in a very few minutes or seconds more, the heat continuing incessantly to be communicated from the air around. Were this really the case, the consequences of it would be dreadful in many cases; for even as things are at present, the melting of great quantities of snow and ice occasions violent torrents and great inundations in the cold

countries, or in the rivers that come from them. But were the ice and snow to melt as suddenly as they must necessarily do, were the former opinion of the action of heat in melting them well founded, the torrents and inundations would be incomparably more irresistible and dreadful. They would tear up and sweep away everything, and that so suddenly, that mankind should have great difficulty to escape from their ravages. This sudden liquefaction does not actually happen; the masses of ice or snow melt with a very slow progress, and require a long time, especially if they be of a large size, such as are the collections of ice and wreaths of snow formed in some places during the winter. These, after they begin to melt, often require many weeks of warm weather before they are totally dissolved into water. This remarkable slowness with which ice is melted enables us to preserve it easily during the summer, in the structures called ice-houses. It begins to melt in these as soon as it is put into them; but, as the building exposes only a small surface to the air, and has a very thick covering of thatch, and the access of the external air to the inside of it is prevented as much as possible, the heat penetrates the ice-house with a slow progress, and this, added to the slowness with which the ice itself is disposed to melt, protracts the total liquefaction of it so long, that some of it remains to the end of summer. In the same manner does snow continue on many mountains during the whole summer, in a melting state, but melting so slowly that the whole of that season is not a sufficient time for its complete liquefaction.

"This remarkable slowness with which ice and snow melt struck me as quite inconsistent with the common opinion of the modification of heat in the liquefaction of bodies."

Black goes on to observe that if one applies one's hand to a piece of ice, the sensation of cold indicates that the ice receives heat very rapidly; yet, if a thermometer be applied to the water dripping from the melting ice, the instrument shows this water to be at the same temperature as the ice. "A great quantity, therefore," he says, "of the heat, or of the matter of heat, which enters into the melting ice, produces no other effect but to give it fluidity, without augmenting its sensible heat; it appears to be absorbed and concealed within the water, so as not to be discoverable by the application of a thermometer."

Until the time of Black, then, it had been believed that when heat was applied to ice, the whole of the heat was manifested as a rise in temperature. But Black now demonstrated that during this and similar changes large quantities of heat were absorbed with no change in temperature—or, in Black's own words, were "rendered latent." He explained these (and similar) facts on the

assumption that the matter of heat united with ice to form water—water being a kind of compound formed by the union of ice and the matter of heat, as if

ice + matter of heat = water, and
water + matter of heat = steam.

In his experiments, he took two glass globes containing equal weights (five ounces) of (1) water at 33° F. and (2) ice at 32° F. respectively, placed them in a room of known uniform temperature, and observed how long it took the ice to melt, and how long it took the water to rise from 33° F. to the temperature of the room. The ice melted in 10½ hours; the water rose from 33° F. to 40° F. in half an hour. Thus the former took 21 half-hours and the latter 1 half-hour. Therefore, Black argued, ice absorbs 21 × (40 − 33), i.e., 21 × 7, or 147 "degrees of heat"; but, of these, 8 were required to raise the resulting water to the final temperature of 40°. Hence, the melting alone required 139 "degrees of heat." Converting to the Centigrade scale, this corresponds to a value of $\frac{5}{9}$ × 139, or 77, which approaches very closely the now accepted value of 80.

Black carried out other experiments by adding known masses of ice and warm water, at known temperatures, and observing the resultant temperature. These experiments gave similar results. He then investigated the analogous changes which occur when water is converted into steam. He placed some water at 50° F. in flat tins on red-hot plates. The water began to boil in 4 minutes, and completely boiled away in 20 minutes. Hence, the conversion into steam required (212 − 50) × 20/4 = 162 × 5 = 810 "degrees of heat." Other experiments gave the results 830 and 750; with steam flowing through a condenser, Black obtained the value 739, which he raised to "not less than 774 degrees," after allowing for experimental errors. The correct value would be 967 "degrees of heat" in Black's system. Or, expressing the figures in our modern units, Black obtained a value of 450 where the correct figure is 538—but accuracy in this determination was not so easy with this method as with the experiments on the melting of ice. All these experiments were made in 1762, except those with steam flowing through a condenser, which were made (jointly with Irvine) in 1764. Black called the heat which he had measured in this way "latent heat." (See Black's *Lectures on the Elements of Chemistry*, ed. by J. Robinson, Edinburgh, 1803, Vol. I, pp. 116 ff., 157 ff., 171 ff.)

In 1764, some weeks after Black and Irvine's experimental determination of the latent heat of vaporization of water, Watt made similar experiments with a smaller and more suitable apparatus and "the medium result of these trials gave 825° for the heat contained in

the steam" (Black's *Lectures*, I, p. 173). Later, wrote Black, "Mr. Watt informs me, that he has observed as exact coincidence between the heat rendered latent in the vapour, and that which emerges from it, as can be desired; and that the heat obtainable from steam, capable of sustaining the ordinary pressure of the atmosphere, is not less than 900° of Fahrenheit's scale, and that it does not exceed 950" (*ibid.*, p. 174). Watt's experiments were carried out in the course of his studies on the steam engine.

Black also explained the phenomenon of supercooling of water, i.e., the cooling of water below its freezing-point (32° F.) without the occurrence of freezing. When water was carefully cooled out of contact with the external air and where it suffered no mechanical disturbance, it could be cooled 7 or 8 or even 10 degrees below its freezing-point without solidifying. "If the water be now touched, ever so gently, with a slender spicula of ice, formed by crystallization, or a flake of dry snow, it instantly shoots into beautiful spiculae, which rapidly form and branch out in all directions, and the thermometer, which had been left in it, rises slowly to the 32nd degree" (*Lectures*, I, p. 130). In these circumstances of careful cooling and freedom from mechanical disturbance, the water, said Black, retains its latent heat, but disturbance, or the addition of an ice-crystal, "occasions the extrication of a part of the latent heat, which now becomes sensible heat, and that part of the water which thus loses its latent heat is at the same time changed into ice. But the heat thus extricated at once being in greater quantity than what is extricated in any one moment in the common process of congelation, it is more conspicuous, by suddenly increasing very remarkably the sensible heat of the materials, and limiting the quantity of the ice that is thus suddenly formed" (*ibid.*, p. 130).

Reflecting on the results of Black's researches in heat, Irvine argued that the distinction between latent heat and sensible heat was unnecessary. Whenever a change of state occurred there was a sudden change in thermal capacity, and a corresponding amount of heat could enter or leave the substance without effect upon the thermometer reading (Black's *Lectures*, I, p. 194). Irvine was further led to suppose that the specific heats of bodies increased with temperature. From this he inferred that the specific heat of water should be greater than that of ice and that, if this were so, it would explain why the temperature of ice could not be raised during melting (*Essays*, pp. 51–2). The results of experiments in which he had found the thermal capacity of ice to be only 0·8 of that of water, Irvine held to be strong confirmation of this theory.

Cleghorn applied his theory of heat to account for differences in heat capacity as between different substances and different states

of the same substance. The particles of heat, he supposed, were attracted with differing intensities by different substances, which thus absorbed the particles in various proportions. A given body attracted particles of heat until its attraction was in equilibrium with their mutual repulsion, and then it attracted no more. However, if this body, say water, was suddenly converted into vapour, the heat particles were then spread through a greatly increased space, and their repulsion was thus greatly diminished. Hence each particle of the water, its attraction for the particles of heat being undiminished, could now gather round itself many more particles of heat in this greater space before their mutual repulsion balanced its own attraction for them (Cleghorn's *Disputatio*, etc., 1779, and Black's *Lectures*, I, pp. 195–6). To Black both Irvine's and Cleghorn's theories were unsatisfactory, since they regarded the heat-changes in fusion and evaporation not as the causes, but as the consequences of fusion and evaporation (Black, *loc. cit.*).

D. The Development of Calorimetry

LAVOISIER AND LAPLACE

After Black, the next work of importance was done by Lavoisier and Laplace, whose *Mémoire sur la Chaleur* was read in 1783 and published in 1784 in the (antedated) *Mémoires de l'Académie Royale des Sciences* for 1780. The authors begin by pointing out that physicists are not agreed as to the nature of heat. Some regard it as a fluid, all-pervading, and contained in, or combined with, bodies according to their varying disposition to contain it or to combine with it. Others regard it as the result of the oscillatory motion of the small parts of matter. They themselves hold no brief for either of these views, since some facts agree with one view and others support the alternative view. But the conservation of *chaleur libre* (i.e., "free heat" as opposed to latent heat) in the simple mixing of bodies, is independent of any such hypothesis, and is admitted by all physicists.

Lavoisier and Laplace adopt as the unit of heat that quantity that raises the temperature of one pound of water by one degree. "Capacities for heat" or "specific heats"[1] of various substances are, therefore, expressed as the quantities of heat required to raise an equal mass of them by an equal number of degrees of temperature. They recognize that "specific heats" are not necessarily constant at all points of the temperature scale; but they propose to regard

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[1] The term "specific heat" (*Chaleur spécifique*) first appeared in Magellan's *Essai sur la Nouvelle Théorie du Feu Élémentaire et de la Chaleur des Corps* (1780), where it is used to denote the total heat in unit mass of a substance at a given temperature.

them as nearly constant from zero to eighty degrees on Réaumur's thermometer. As an example of their method, they give the following case: Take a pound of mercury at 0° and a pound of water at 34°. Mix them. A uniform temperature of 33° is produced. The heat lost by the water has raised the temperature of the mercury by 33°. Therefore, to raise mercury to a given temperature needs only $\frac{1}{33}$ of the heat necessary to raise an equal mass of water to the same temperature; and the specific heat of mercury is $\frac{1}{33}$ of that of water. This can be generalized as follows: Let m represent the mass of the hotter of two bodies in pounds, let a represent its temperature, and q the heat necessary to raise one pound of it by one degree. For the other body, let these values be m', a', q', respectively. Let b be the temperature when the bodies are mixed and have a uniform temperature. The heat lost by the body m is $m \cdot q \cdot (a - b)$. The heat gained by the body $m' = m' \cdot q' \cdot (b - a')$. These are equal. Therefore $mq(a - b) = m' \cdot q' \cdot (b - a')$. Whence it follows that $q/q' = m'(b - a')/m(a - b)$, and it is not necessary to know q and q'.

The method of mixture is, however, impracticable in many cases, and not of a high order of accuracy. For example, when the density of the two substances differs widely it is difficult to ensure that all parts attain the same temperature. Moreover, when the substances interact chemically, an intermediate reference body has to be used, and, in some cases, several intermediates; and, as this increases the number of determinations, so it increases the errors. The method is also inapplicable to determinations of the heat of chemical reactions, and of combustion and respiration, which are most important for the formulation of a proper theory of heat.

Accordingly they report that they have devised a new apparatus, or calorimeter, for measuring heat by noting how much ice it melts (see Illustr. 90).

In order to determine the specific heat of a solid body, this body is raised to a known temperature, placed quickly in the calorimeter, and left there until its temperature falls to zero. The water produced is collected and weighed. This mass of water, divided by the product of the mass of the body and the number of degrees of temperature it was originally above zero, is proportional to the specific heat of the body. Actually the value thus given is the quantity of ice melted by a unit mass of the body in cooling through one degree of temperature. But, as is explained later in the memoir, this has to be multiplied by 60 to give the specific heat required—60, because one pound of water in cooling from 60° to zero melts one pound of ice; in other words, 60 is the value of the latent heat of fusion of ice on the Réaumur scale. Curiously enough, Lavoisier and Laplace
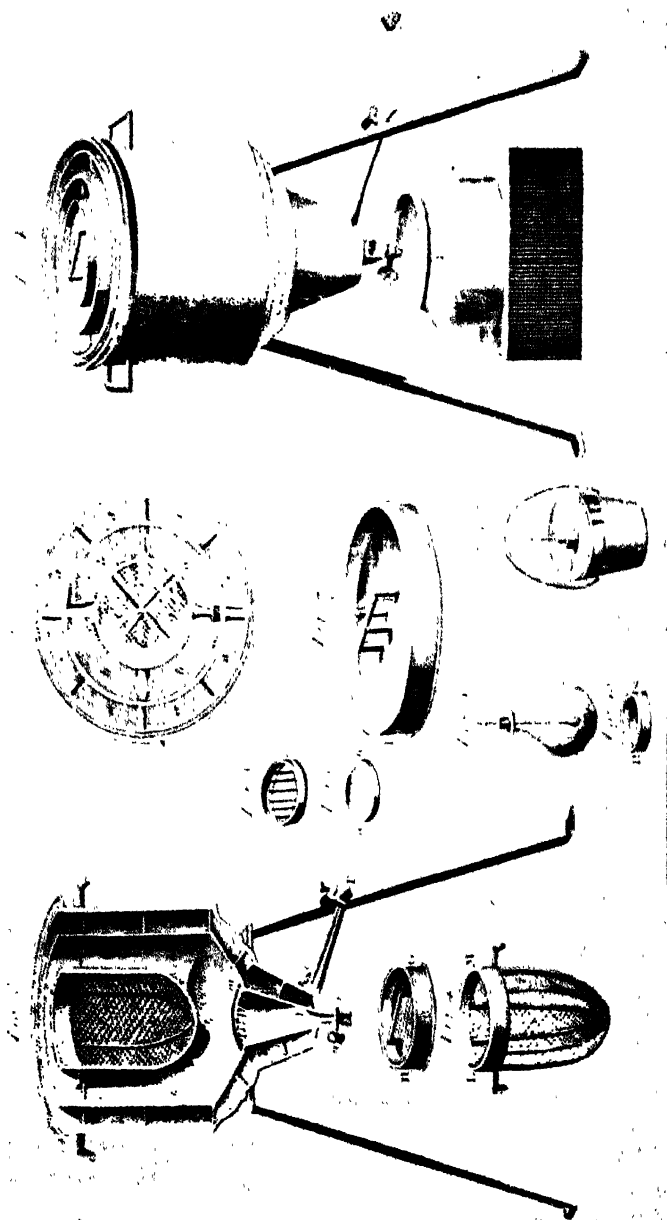
The Calorimeter of Lavoisier and Laplace

never mention "latent heat"; and the argument by which this multiplier is justified is so roundabout as to suggest that they were trying to avoid the admission that their method depended on Black's discovery of latent heat.

Lavoisier and Laplace likewise showed how to determine the specific heats of liquids, heats of combination, the degree of cooling produced by certain solutions of salts, heats of fusion, the heat produced in respiration and combustion, and the specific heats of gases. Fluids were treated in much the same way as solids, except that they had to be enclosed in vessels, and corrections had to be made for the heat lost by the vessels in cooling. In determinations of the heat of combination of several substances, the substances and the several vessels containing them were all cooled to zero. They were then mixed and put into the calorimeter, and kept there until the mixture had cooled to zero. The water produced measured the heat evolved. The degree of cold produced by the solution of salts was determined by raising each of the substances to the same temperature, mixing them with water in the calorimeter, measuring the quantity of ice melted, and then heating the mixture to a known temperature, putting it in the calorimeter, letting it cool to zero, and again noting the amount of ice melted. From the latter, one could determine the proportion between the range of temperature and the mass of ice melted, and hence the temperature corresponding to the mass of ice melted in the first part of the experiment. The difference between this temperature and the temperature to which the bodies had been raised gave the degree of cold produced in the process.

Heats of fusion were determined as follows: Let m be the melting-point of the body investigated. Heat it to $m - n$ degrees, and then put it in the calorimeter. Let the amount of ice melted while the body cools to zero $= a$. Heat it to $m + n'$ degrees, and repeat. Let the quantity of ice melted $= a'$. Heat it to $m + n''$ degrees, and repeat. Let the quantity of ice now melted $= a''$. Therefore, $a'' - a'$ is the amount of ice melted by the body when liquid in cooling through $n'' - n'$ degrees. It follows that, in cooling from n' degrees, the body would melt a quantity of ice $= n'(a'' - a')/(n'' - n')$. Now, the body in cooling from m degrees in the solid state would melt a quantity $= ma/(m - n)$ of ice. Let x be the quantity of ice melted by the heat evolved by the body in passing from liquid to solid. Then the total mass of ice melted by the body when heated to $m + n'$ degrees will be $n'(a'' - a')/(n'' - n') + x + ma/(m - n)$, which $= a'$. Therefore,

$$x = (n''a' - n'a'')/(n'' - n') - ma/(m - n).$$

For accuracy, n and n' must not be large. The method, as the authors point out, gives, not only the value of x, but also the values of the specific heats of the substance in the solid and liquid state.

Heats of combustion and respiration were determined by burning substances, or allowing animals to breathe, in the calorimeter, fresh air being supplied from outside by a suitable arrangement. The experiments necessitated reducing the temperature of the combustibles, or the animals, as nearly as possible to zero. The specific heats of gases, they suggest, might be determined by passing a current of gas through a worm-tube placed in the calorimeter, the temperatures of the gas at entrance and exit being observed with thermometers placed in the gas at these points. The thermometers indicated the fall of temperature of the gas; the calorimeter gave the mass of ice melted; the mass of gas passed was easily determined; and from these figures the specific heat could be evaluated.

Having thus expounded the main methods and results of calorimetry, Lavoisier and Laplace draw attention to the fact that these results give no information about the *absolute* quantities of heat in substances, but merely the relative amounts of heat required to raise their temperatures by the same number of degrees. Specific heats, in other words, are only the ratios of the differentials of the absolute heats; and to suppose that these differentials are proportional to the absolute heats would be unwarranted. The zero of the thermometer clearly does not exclude the presence of a considerable amount of absolute heat. Far from it. Lavoisier and Laplace attempted to determine at least the relation between the absolute heat of a substance at zero and its gain in heat when its temperature has been raised by one degree. The attempt was based on a study of the heat evolved in the combination of two substances. But the results obtained were conflicting and unsatisfactory.

The final section of the memoir is devoted to experiments on the heat evolved in combustion and respiration. The authors burned a weighed amount of charcoal in a measured volume of "pure air" (oxygen) in a bell-jar inverted over mercury. They measured the amount of fixed air produced (by absorbing it with alkali and noting the diminution of volume) and the amount of "pure air" remaining. They had previously burned a known weight of charcoal in the calorimeter and determined the mass of ice melted. Combining the results, they showed that burning one ounce of charcoal consumed 3·3167 ounces of "pure air" and formed 3·6715 ounces of "fixed air"; that in this combustion the "alteration" of one ounce of "pure air" melts 29·547 ounces of ice; and that the formation of one ounce of "fixed air" melts 26·692 ounces of ice. They present this result with the warning that only one experiment has been

carried out, and that they are more concerned with informing physicists of their method than with the result of this experiment. It is to be noted that they speak of the result as "the quantities of heat disengaged by the alteration of an ounce of pure air in the combustion of charcoal." A similar experiment on the combustion of phosphorus in "pure air" showed that an ounce of "pure air" in being absorbed by phosphorus melts 68·634 ounces of ice. They express some surprise at the fact that "the heat set free by pure air, when it is absorbed by phosphorus, is almost 2⅓ times as much as when it is changed into fixed air." The terms in which this result is expressed, and its consequences discussed, depend on Lavoisier's theory that airs and vapours owe their aeriform state to the large amount of heat with which they are combined. "Pure air," or oxygen, was considered by him to contain a very large amount of heat, which it lost almost completely when it passed into a concrete state, as in the calcination of metals and the combustion of phosphorus, sulphur, etc.—but a considerable part was retained in "fixed air." In the case of the combination of "pure air" with "nitrous air," there was an apparent exception—a very small amount of heat was released. Therefore, nitric acid (the product) and nitre must contain a great quantity of this heat—which appeared subsequently in detonations. Thus, to Lavoisier, the heat evolved in these changes was the heat contained in the "pure air" and set free by that air assuming the solid state in combination. We know now that the amount of heat thus evolved is only a fraction of the heat of these chemical reactions —but Lavoisier had no knowledge of chemical energy.

The authors then carried out similar experiments on respiration. They placed guinea-pigs in "pure air" and "common air" in the bell-jar over mercury, and collected the "fixed air" produced in caustic alkali, weighed before and after the experiment. The mean of several experiments gave a result of 224 grains of "fixed air" produced in 10 hours. From the experiment with burning charcoal, previously made, they calculated that the formation of 224 grains of "fixed air" would melt 10·38 ounces of ice. "This quantity of melted ice, therefore, represents the heat produced by the respiration of a guinea-pig in 10 hours." Moreover, the animal had the same heat at the end as at the beginning of the experiment, since the internal heat of animals is always nearly the same. The ice melted, therefore, represents the heat lost by the animal, and renewed by it during the same period by its vital functions. "We can look upon the heat which is evolved in the change of 'pure air' into 'fixed air' by respiration as the chief cause of the conservation of animal heat; if other causes assist in maintaining it, their effect is small." "Respiration is therefore a combustion, although a very slow one,

but otherwise thoroughly similar to that of charcoal; it occurs inside the lungs, without evolving sensible light, because the matter of fire, having been set free, is at once absorbed by the moisture in those organs; the heat developed in this combustion is communicated to the blood flowing through the lungs, and thence is spread throughout the animal system. Thus the air we breathe serves two purposes equally necessary for our preservation; it removes from the blood the base of fixed air, the excess of which would be highly injurious; and the heat evolved in the lungs by this combination makes good the continual loss of the heat we impart to the atmosphere and neighbouring bodies.''

E. Absolute Zero

From his determination of the specific heat of ice, Irvine was led to an ingenious attempt to evaluate the absolute zero of temperature. He describes the method as follows: "The number of degrees by which a body becomes hotter or colder [change in absolute heat-content], upon changing its form, will be as the capacity of the solid to that of the fluid, supposing the quantity of heat x in them, to be the same in all, or to produce the same expansion [in the thermometric liquid] when the change takes place, or their freezing-point to happen at the same degree of sensible heat [temperature]. Let the quantity of heat in them be expressed by 100. If the capacity of the solid be to that of the fluid . . . as 1 : 2, then there must be twice as much heat in the body when fluid as there was in it when cold [solid], or 100 degrees of heat must have become latent. If the capacities be as 1 : 3, then the whole heat, when the body is fluid, must be 300, and the latent heat 200. . . . And universally the whole heat in the solid being 100, the latent heat in the fluid will be equal to the difference of the capacities of the solid and fluid, divided by the number expressing the capacity of the solid, and multiplied by the whole heat of the solid" (*Essays*, pp. 117–18). Irvine's son formulated the proposition mathematically: If a and b are the specific heats of water and ice respectively, l the latent heat of fusion of ice, and x "the absolute heat of the solid body expressed in degrees," then

$$l = \left(\frac{b-a}{b}\right) \times x \quad (\textit{Essays, p. 122}).$$

Inserting Irvine's own value for the specific heat of ice (0·8) we have

$$140 = \frac{0\cdot 8 - 1}{0\cdot 8} \times x$$

whence $x = -140 \times 4 = -560$, which gives the absolute zero as $-528°F.$,—a result not mentioned by Irvine, who gives the figure obtained by his father as $-900°$ (*Essays*, pp. 127 and 137). It is probable that he obtained this figure from the results of other determinations of the specific heat of ice. Irvine also applied this method to the heats of solution and chemical reaction, which he regarded as latent heat; and from the heat evolved by the addition of sulphuric acid to water, and the specific heats of these two substances, he obtained the same value, $-900°$ F., for the absolute zero (*Essays*, p. 127) as in the case of ice and water.

Crawford also applied the method to the thermal data of the combustion of "dephlogisticated air" and "inflammable air" and obtained a value of $-1500°$ F. for the absolute zero (*Experiments and Observations on Animal Heat*, 2nd ed., 1788, p. 267).

The younger Irvine (*Essays*, p. 55) quotes a value for the specific heat of ice as $0·9$, which he questionably ascribes to Crawford; and he uses this figure to evaluate the absolute heat in ice at $32°$ F. as $1260°$ (*ibid.*, p. 122).

Gadolin also worked on this problem. Calculating from Black's estimate of $147°$ F. for the latent heat of the fusion of ice, and Wilcke's estimate of $72\frac{1}{8}°$ C. for the latent heat absorbed in the melting of snow, and taking $\frac{9}{10}$ as the specific heat of ice, he found that the zero of temperature should, according to these data, be at $-817°$ C. or $-722°$ C., whereas his own experiments pointed to $-800·6°$ C. Subsequently, however, he estimated the specific heat of snow at $0·52$, so that the zero of temperature appeared to be $-170·6°$ C., whereas similar determinations with wax suggested a value of $-480°$ C. for the zero of temperature (*Nova Acta Reg. Soc. Sci. Upsala*, 1792, Vol. V, 1). From the inconsistency of these results Gadolin concluded that the method was unsatisfactory, and that the specific heat of substances probably varied with their temperature, and was not proportional to their total heat.

F. Measurement of Thermal Expansion

The precise measurement of the thermal expansion of bodies, solid and liquid, afforded problems which attracted a number of eighteenth-century investigators.

Brook Taylor (*Phil. Trans.*, 1723) showed that the expansion of a thermometric liquid was proportional to the increase of heat in a surrounding bath. He used a linseed oil thermometer with divisions carefully marked to give equal volumes between the marks on the stem, and he used for the experiment two vessels of thin tin, of the same size and shape, and each of about one gallon capacity. "Then

(observing in every Trial, that the Vessels were cold, before the water was put in them, as also that the Vessel I measured the hot water with, was well heated with it) I successively fill'd the Vessels with one, two, three, etc., Parts of hot boiling water and the rest cold; and at last with all the water boiling hot; and in every Case I immersed the Thermometer into the water, and observed to what Mark it rose, making each Trial in both Vessels for the greater Accuracy. And having first observed where the Thermometer stood in cold water, I found that its rising from that Mark, or the Expansion of the Oil, was accurately proportional to the Quantity of hot water in the Mixture, that is, to the Degree of Heat."

The linear expansion of solids was investigated by John Ellicott
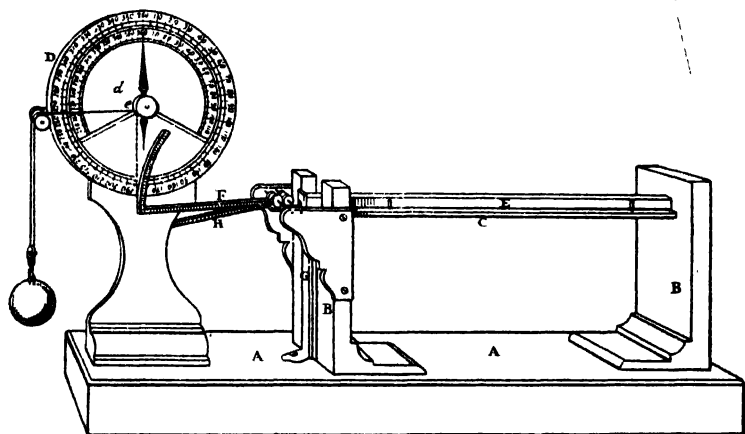
Illustr. 91.—Ellicott's Instrument for Measuring Thermal Expansion

(*Phil. Trans.*, 1736), who devised "an Instrument for measuring the Degrees of the Expansion of Metals by Heat," in which the expansion of a bar of metal was measured against the expansion of a standard bar of iron at the same degree of heat, on which it was laid. By suitably connected levers and pulleys the differential expansion of the bars operated a pointer which moved over a graduated disc, somewhat as in the familiar lecture-experiment. An expansion of 1/7200 inch caused the pointer to move over one division of the scale.

"A new Pyrometer" of a similar type to Ellicott's was devised by Smeaton (*Phil. Trans.*, 1754, pp. 608 f.). In this instrument the expansion of a bar of any given metal was compared with that of a standard bar or "basis" of brass, whose expansion over a given range of temperature was in turn ascertained once for all by comparison

with a standard bar of wood (white deal or cedar). The bars used were 2 feet 4 inches in length (Illustr. 92). The brass bar formed the permanent base of the instrument and terminated in upright standards
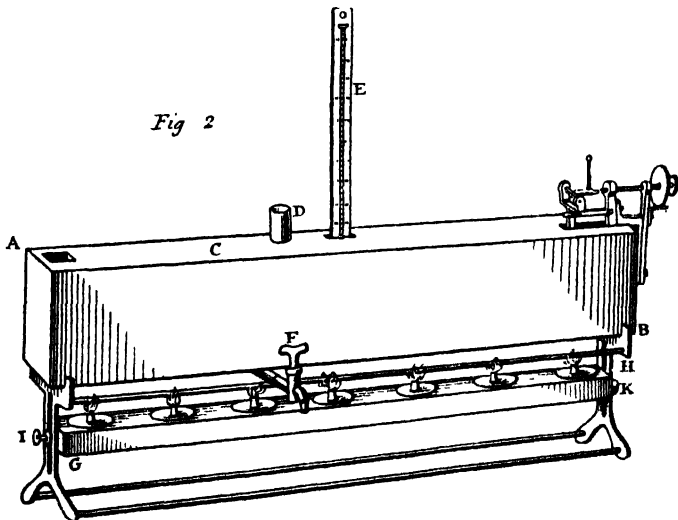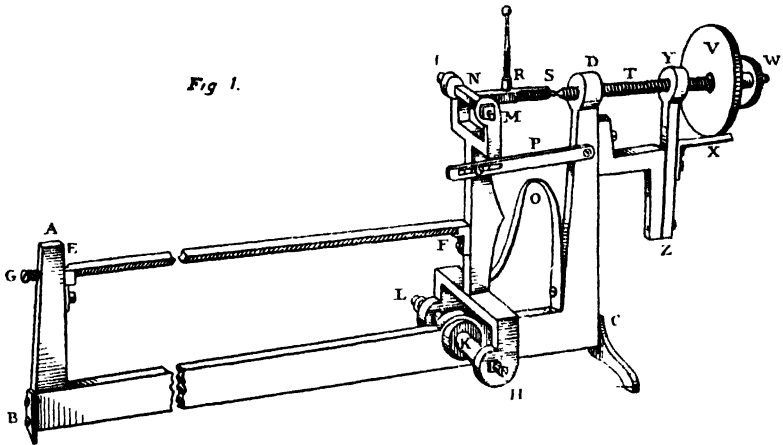
Illustr. 92.—Smeaton's Pyrometer

on which the bar to be tested was supported. Both bars could be immersed in a bath heated by lamps to the desired temperature, which was read on a thermometer. The wooden bar had its ends protected from moisture and steam by covering them with brass,

and the whole bar was varnished and wrapped from end to end with "coarse flax." A sensitive micrometer-screw was used to measure the difference of expansion of the basis and bar over a given range of temperature, and the total expansion of the bar was obtained by adding this difference to, or subtracting it from, the expansion of the basis over that range, as measured against the wooden standard. The wooden bar was only brought near the apparatus when required, and was always kept outside the bath. Its expansion was small in comparison with that of the bars, but was allowed for as follows: the interval of time between placing the wooden bar in position on the instrument and taking the readings was recorded by a seconds-watch, or otherwise; and, after an equal interval, a second measurement was made, and so on, to a third and a fourth interval. "The three differences of these four measures will be found nearly to tally with three terms of a geometrical progression, from which the preceding term may be known, and will be the correction; which, if apply'd to the measure first taken, reduces it to what it would have been if the wooden bar had not expanded during the taking thereof." Smeaton claimed that his results were reproducible to within one-twenty-thousandth part of an inch. From his experiments he obtained values for the coefficients of expansion of iron, steel, antimony, bismuth, copper, brass, lead, tin, zinc, various alloys, and glass.

Ramsden devised a method for measuring the expansion of a metal bar against standard bars maintained at a definite temperature. His apparatus was used by Roy (*Phil. Trans.*, 1785) in determining the expansion of the rods used to measure a base-line on Hounslow Heath. Three parallel troughs (Illustr. 93) were taken, over five feet long, the outer ones each containing an iron bar kept at constant temperature by a packing of pounded ice, and the inner one, suitably heated below when necessary, containing the bar to be tested, which was fixed at one end. The change in length of the middle bar was determined by means of the following optical system: An eyepiece with cross-wires was fixed to each end of the outer bar A, and lenses were fixed to each end of the middle bar: these served as object-glasses to the eyepieces on A. Each end of the outer bar B carried cross-wires illuminated from behind by mirrors. The two telescopic systems formed by the eyepieces on A and the object-glasses on the middle bar were adjusted so that the images of the cross-wires on B coincided with the cross-wires of the eyepieces on A, when the three troughs containing the bars were packed with ice. The ice in the middle trough then gave place to hot water, maintained at a steady temperature by the lamps below. The bar expanded, and the free end moved outwards, carrying with it the
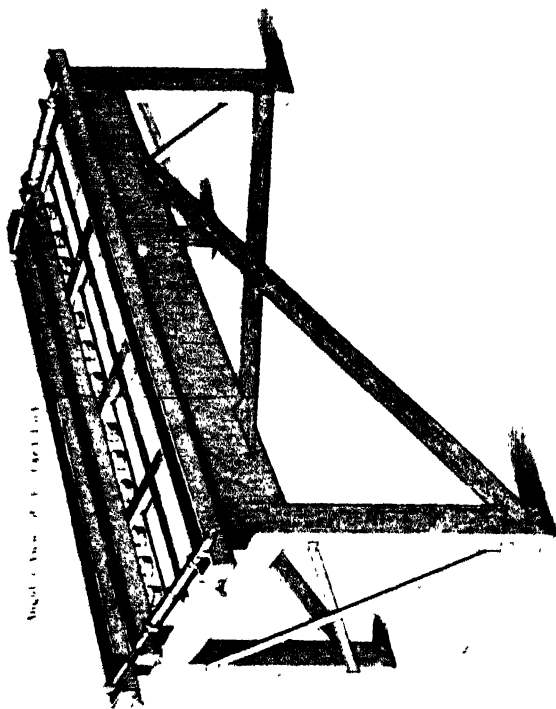
Ramsden's Pyrometer

affixed object-glass. When conditions were steady, the eyepiece on the end of the bar A was moved until coincidence of the image and cross-wires again occurred, the distance traversed being measured by the fine micrometer screw to which it was attached. The increase in length of the middle bar between the two temperatures was then calculated by simple proportion.

G. HEAT AND WEIGHT

The prevalence of the caloric theory of heat was largely the result of the observations of such phenomena as the expansion of bodies when heated, and the increase in the weight of metals on calcination. It was natural in the circumstances to suppose that heat was some kind of material substance and that it had weight. Accordingly various attempts were made in the course of the eighteenth century to determine the concomitant variations, if any, between the temperature and the weight of bodies. The experimental results obtained appeared to be conflicting. Some experimenters found that an increase in the temperature of a substance was followed by a slight gain in weight; some observed a slight loss in weight; and others could detect no variation in weight with variation of temperature. The most carefully conducted experiments were those of Rumford, whose results were negative, as Boerhaave, Black, and some others had anticipated. Of the work done on this problem the most important stages may be summarized as follows, in chronological order.

Boerhaave (*Elementa Chemiae*, Leiden, 1732, I, pp. 259–60; and *New Method of Chemistry*, trans. P. Shaw, 2nd ed., London, 1741, I, pp. 285 f.) took a mass of iron weighing 5 lbs. 8 ozs. He weighed it when cold, re-weighed it when it was red-hot, and again when it had once more become cold. The weight remained constant. The same result was obtained with a mass of copper when tested under the same conditions.

Buffon (*Histoire Naturelle*, Supplement, Paris, 1775, II, pp. 11–13) found that a "white hot" mass of iron weighed 49 lbs. 9 ozs.; but that, when cooled to atmospheric temperature (then near the freezing-point) it weighed only 49 lbs. 7 ozs. Experiments with other masses of iron gave similar results.

Roebuck (*Phil. Trans.*, 1776, p. 509) repeated Buffon's experiments with smaller masses and more sensitive balances. He found that 1 lb. of iron at a white heat weighed nearly 1 grain less when cooled, but that a mass of 5 pennyweights was slightly heavier when cold than when hot; that a piece of hot copper, weighing about 1 lb., lost 4 grains when cooled, although this was accounted for by loss of

scales from the metal; that a mass of wrought iron weighing 55 lbs., gradually cooled from a white heat in 22 hours, gained over 6 pennyweights; and that the scales of such iron weighed more when cold than when hot, 2 ozs. 8 pennyweights increasing by 5 grains; but that masses of pure silver (about 2 lbs. 10 ozs., when hot) gained 5 grains on cooling.

Whitehurst (*ibid.*, p. 575) was likewise unable to confirm Buffon's results. Pennyweights of gold and of iron lost slightly in weight when heated to redness; but the gold recovered its weight when cooled, and the iron showed a slight gain. He concluded that the air rarefied by these heated metals on one side of the balance might cause an up-draught and so produce the effects observed, whereas in Buffon's experiments the large mass of hot metal might have led to unequal expansion of the opposite arms of the balance, and thus have given rise to the differences observed. Fordyce (*ibid.*, 1785, p. 361) found that a mass of ice when melted to water showed a decrease of weight. He took 1700 grains of New River water in a glass vessel weighing 451 grains. When sealed off, the apparatus and contents weighed $2150\frac{31}{32}$ at 32° F. In successive partial freezings of the contents, the total weight gradually increased until, when freezing was complete, the total gain was slightly over $\frac{3}{16}$ grain, and the temperature had fallen to 12° F. After the temperature had risen to 32° F. and the ice had completely re-melted, the apparatus was found to have recovered its original weight.

Black's view, that heat had no weight (*Lectures*, Vol. I, pp. 48 f.), was based mainly on his interpretation of the experimental results obtained by Whitehurst and Fordyce.

Rumford (*Phil. Trans.*, 1799, p. 179) now directed his attention to this problem. He took two thin glass flasks as nearly alike as possible, containing equal weights of distilled water and of weak spirits of wine respectively. They were sealed off and hung, each from one arm of a balance in a room at 61° F. The apparatus was then moved into a cooler room at 29° F. At the end of 48 hours the water was frozen, and an addition of 0·134 grains was now necessary to restore equilibrium. Then the apparatus was returned to the room at 61° F., and when the ice had melted, Rumford found that the original weight had been recovered.—The balance, when tested again after the experiment, was quite accurate.

The only explanation he could think of was that the water when freezing had lost a great proportion of its latent heat. Now, "if the loss of latent heat added to the weight of one body, it must produce the same effect on another, and consequently, the augmentation of the quantity of latent heat must—in all bodies—and in all cases—diminish their apparent weights." However, when he repeated the

previous experiment with mercury in place of spirit of wine and at the temperatures of 61° F. and 34° F., he found no difference in the weights, although the water must have lost much more heat than the mercury, since their specific heats were as 1000 to 33. And now Rumford suspected that some accidental cause (such as deposition of atmospheric moisture on the flasks, or minute local currents of air due to small differences of temperature) might have given rise to the apparent gain in weight in the first experiment. He therefore took three flasks, containing equal weights of water, spirits of wine, and mercury, respectively. They were sealed and put in the warm room at 61° F. for 24 hours. The temperatures of the water and the spirits, recorded by small thermometers fixed in the flasks, were identical. All moisture was then carefully wiped from the surfaces of the flasks, which were now weighed and balanced by attaching pieces of silver wire to the necks of the lighter flasks. All were then placed in a cold room at 30° F. for 48 hours, at the end of which time they were all found to have reached the same temperature (29° F.) and to have undergone no change in weight. Moreover, their weights remained the same when they were returned to the warm room. Several repetitions of the experiment showed the same result. His first result, Rumford was now satisfied, must have been due to some such accidental cause as he had surmised.

"Having determined," says Rumford, "that water does not acquire or lose weight, upon being changed from a state of *fluidity* to that of *ice*, and vice versa, I shall now take my final leave of a subject which has long occupied me, and which has cost me much pains and trouble; being fully convinced (from the results of the above-mentioned experiments) that if heat be in fact a *substance*, or matter —a fluid *sui generis*, as has been supposed—which, passing from one body to another, and being accumulated, is the immediate cause of the phenomena we observe in heated bodies (of which, however, I cannot help entertaining doubts), it must be something so infinitely rare, even in its most condensed state, as to baffle all our attempts to discover its gravity. And, if the opinion, which has been adopted by many of our ablest philosophers, that heat is nothing more than an intestine vibratory motion of the constituent parts of heated bodies, should be well founded, it is clear that the weights of bodies can in no wise be affected by such motion."

If heat were a substance and had weight, that weight would have been more easily detected in Rumford's experiments than in any others, because in freezing the water had lost 140° F., an amount which would raise the temperature of an equal mass of gold from freezing-point to 140 × 20 or 2800° (a bright red heat), since the specific heat of gold was one-twentieth that of water. "It appears,

therefore," concludes Rumford, "to be clearly proved, by my experiments, that a quantity of heat equal to that which 4214 grains (or about 9¾ ozs.) of gold would require to heat it from the temperature of freezing water to be red hot, has no sensible effect upon a balance capable of indicating so small a variation of weight as that of one-millionth part of the body in question; and if the weight of gold is neither augmented nor lessened by one-millionth part, upon being heated from the point of freezing water to that of a bright red heat, I think we may very safely conclude, that all attempts to discover any effect of heat upon the apparent weights of bodies, will be fruitless."

H. The Mechanical Theory of Heat

RUMFORD

Following the experimental work on heat by Black, Lavoisier, and Laplace, the next important contribution to the subject was made by Sir Benjamin Thompson, Count Rumford (1753–1814).

Born in Massachusetts, Thompson started life as apprentice to a Salem merchant; later, after attending lectures at Harvard, he became a schoolmaster. He was appointed to a school at Rumford (now called Concord), and there he married the squire's widow, and settled down to farm the estate which thus came into his possession. He had always shown great interest in mechanical contrivances, and he now entered upon a course of investigations on the properties of gunpowder. During the War of Independence Thompson suffered imprisonment for his sympathy with the English Government; in the end he crossed the Atlantic and was appointed to the Colonial Office. In London, his scientific interests won for him the friendship of Sir Joseph Banks, and in 1779 he was elected a Fellow of the Royal Society. In 1784, after a short spell of military duty in America, Thompson (now a knight) entered the service of the Elector of Bavaria. During the next eleven years, which he spent at Munich, he was actively engaged in reorganizing the Bavarian army, and he also attacked the problem of pauperism with great energy, seeking a radical but humane solution in the establishment of national workhouses. Thompson was created a Count of the Empire in 1791, taking his title from his American estate. He left Munich in 1795, and for a time turned his attention to the social problems of Ireland. Soon afterwards, however, he was recalled to Bavaria, whose neutrality was threatened by the contending French and Austrian armies, and he commanded the garrison of Munich throughout the crisis. The establishment of the Royal Institution was Rumford's next preoccupation; but in 1805 he married the
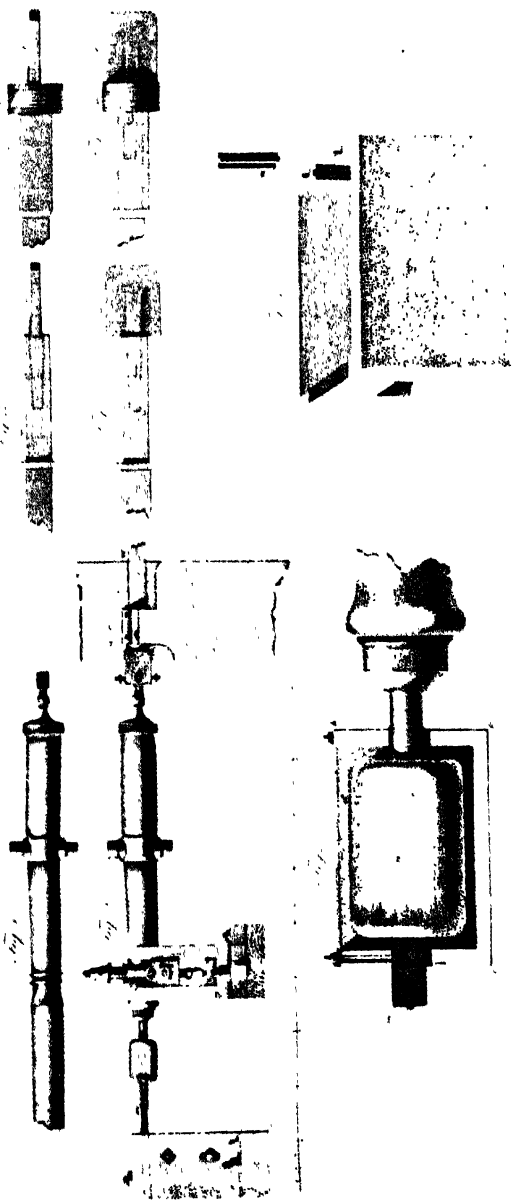
Rumford's Apparatus

FIG. 2.—A cannon fixed in a boring machine: W is an iron bar worked by machinery (not shown) to turn the cannon round its axis; m is another strong bar attached to a blunt lever which is forced against the short hollow cylinder (enlarged in Fig. 3) connected to the end of the cannon. FIG. 3.—At de is a hole for a thermometer; ghik is a wooden box (shown also in Fig. 4) encasing the hollow cylinder. FIGS. 5, 6.—The blunt borer, n, is joined to the bar, m. FIGS. 7, 8.—A piston, B, closing the mouth of the hollow cylinder.

widow of the chemist Lavoisier, and his last years were spent at Auteuil, where he died in 1814. It was by some observations which he made while supervising the boring of cannon at Munich that Rumford was led to undertake investigations on heat, whereby he became convinced that it is a "mode of motion." Rumford was the founder of the "Rumford Medal" of the Royal Society, awarded for distinction in the useful applications of heat and light, and he was himself the first to win this honour in recognition of his contributions to fuel economy, the design of chimneys, etc.

While engaged in boring cannon at Munich, he had observed with surprise that very large amounts of heat were produced by the action of the boring tool on the gun. The caloric theory explained this as due to the diminished heat capacity of the metallic chips through the squeezing out of their caloric in the process of boring, this caloric then appearing as sensible heat. Calorimetric measurements, however, showed that there had been no alteration produced in the thermal capacity of the chips as compared with that of the bar metal. In an experiment on the production of heat by boring, which was performed in 1798, a brass cylinder was made to rotate against a steel borer (Illustr. 94). The cylinder was placed inside a wooden box, which held 18¾ lbs. of water. This formed a calorimeter, since the amount of heat produced could be measured by observing the rise in the temperature of the water. The temperature rose from 60° F. to boiling-point (212° F.) in 2¾ hours. In Rumford's words, "It would be difficult to describe the surprise and astonishment expressed in the countenance of the bystanders on seeing so large a quantity of water heated, and actually made to boil, without any fire." The heat had evidently been produced by mechanical means alone, but Rumford wondered where it had come from. He says: "As the machinery used in this experiment could easily be carried round by the power of one horse . . . these computations show further how large a quantity of heat might be produced, by proper mechanical contrivance, merely by the strength of the horse, without fire, light, combustion, or chemical decomposition." Since the apparatus was under the water, the heat clearly could not come from the air. Calorimetric measurements showed that there had been no change in the thermal capacity of the chippings. Now, if the caloric theory were correct, there should be some limit to the amount of heat producible from a given mass of metal. Here, however, no limit was apparent. Rumford wrote accordingly: "In reasoning on this subject, we must not forget to consider that most remarkable circumstance, that the source of the heat generated by friction in these experiments, appeared evidently to be inexhaustible. It is hardly necessary to add, that anything

which any insulated body, or system of bodies, can continue to furnish without limitation, cannot possibly be a material substance. It appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in the manner in which the heat was excited and communicated in these experiments, except it be motion. I am very far from pretending to know how or by what means or mechanical contrivance that particular kind of motion in bodies which is supposed to constitute heat is excited, continued, and propagated" (*Phil. Trans.*, 1798, p. 80).

DAVY

Rumford's experiments clearly disposed of the caloric theory of heat in favour of a mechanical interpretation. His scientific contemporaries, however, had a sufficient measure of intellectual inertia to resist the new idea. So the caloric theory survived until the middle of the nineteenth century. Still, Rumford succeeded in converting at least one youthful contemporary to his view of heat—a contemporary, moreover, who was destined to become distinguished later. Humphry Davy carried out some friction experiments with blocks of ice, and with other substances, during the years 1797-99. The results convinced him that "heat cannot be considered as matter," but must be regarded as a "peculiar motion, probably a vibration of the corpuscles of bodies" (*Works*, ed. 1839, Vol. II, pp. 11-14; originally in *Contributions to Physical and Medical Knowledge*, ed. by T. Beddoes, Bristol, 1799, pp. 16-22). The youthful Davy's friction experiments were unsatisfactory in design and execution (see Andrade, *Nature*, 1935, pp. 359 ff.). Some historians have been tempted to give young Davy more credit in this connection than is due to him.

I. OTHER STUDIES IN THE HEAT OF MIXTURES

While Black was pursuing his physical researches in Scotland, similar investigations into the phenomena of heat were being made, in Sweden, by Johan Carl Wilcke. Wilcke's methods and results were less valuable than those of Black; but Wilcke worked quite independently of Black, and his work is of some historical interest. In order to understand Wilcke's mode of dealing with the phenomena of heat it is necessary to consider the work done by certain other Continental physicists in this particular field of enquiry. For he was really the principal member in a succession of workers of whom Morin was the first, and Gadolin was the last.

Jean Baptiste Morin (1583–1656), Professor of Mathematics and Astronomy at the Collège Royal, in Paris, had attempted to discover the law relating to the resulting temperature when hot and cold samples of the same liquid are mixed. At that time heat and cold were still regarded as positive entities, though as opposites; cold was not yet conceived as merely a low degree of heat. Descartes, for instance, regarded heat as consisting of fine fire-like particles; and Gassendi thought of cold as composed of "frigorific" particles. Now, Morin believed that heat and cold were always conjoined, that neither ever existed entirely without the other, though they might co-exist in varying proportions. In the process of mixing, heat and cold were interchanged, not destroyed; action and reaction took place only between the opposite qualities present in greater degree, while those present in less degree became intensified; and the total "virtue" of a number of "degrees of heat" was equal to that of an equal number of "degrees of cold." Morin thought that each of the qualities, heat and cold, had a certain maximum degree, which it could not exceed, and a certain minimum degree, below which it could not fall. And he assumed that the sum of the units of heat plus the units of cold always $= 8$. Thus when the heat and the cold in a substance are equal, then there are 4 units of each of them present. If the heat exceeds the cold, then there may be present 5 units of heat and 3 of cold, or 6 of heat and 2 of cold, or 7 of heat and 1 of cold. If the cold exceeds the heat, then there may be 5 units of cold and 3 of heat, or 6 of cold and 2 of heat, or 7 of cold and 1 of heat. Intermediate fractional proportions are also possible, of course. But according to Morin there can never be, say, 6 units of heat and 3 of cold, or any other proportion giving a total other than 8. Morin himself never says so in so many words, but his arguments imply all this. (See D. McKie and N. H. de V. Heathcote, *The Discovery of Specific and Latent Heats*, London, 1935, pp. 55–59, 149–51.)

Suppose now that a given quantity of water having 2 units of heat and 6 of cold is mixed with an equal volume of water having 4 units of heat and 4 of cold. What degree of heat will the mixture have? It cannot have only 2° of heat, because in that case 2 of the 4 units of heat of the warmer water will have vanished without leaving any effect on the colder water. Nor can the mixture have 4° of heat, for in that case 2 out of the 6 units of cold of the colder water will have disappeared without any effect on the warmer water. But units of heat and cold, according to Morin, cannot be destroyed; they can only alter their proportions. The mixture must consequently have more than 2° and less than 4° of heat. Yet Morin does not think that it will have 3° of heat. He argues that if the mixture had 3° of heat, and consequently 5° of cold, then the 6°

of cold in the colder water would only have reduced the 4° of heat in the warmer water by 1°, whereas the 4° of heat in the warmer water would have reduced the 6° of cold in the colder water by 1°, and such disproportion seemed to him impossible. The mixture must consequently have less than 3° of heat, so as to show proportionate effects for the 6° of cold in the colder water and the 4° of heat in the warmer water. As these are in the proportion of 3 to 2, Morin finally concludes that the mixture will have $2\frac{4}{5}$° of heat and $5\frac{1}{5}$° of cold, so that the 2° of cold which the colder water had above the 4° of cold in the warmer water will have reduced the heat of the warmer water by $\frac{6}{5}$ of a degree, while the 2° of heat which the warmer water had above the 2° of heat in the colder water will have reduced the cold of the colder water by $\frac{4}{5}$ of a degree. The effects produced will thus be in the proportion of 3 to 2. (See his *Astrologia Gallica*, Lib. VIII, Cap. XV, pp. 158 f.) Had Morin consistently followed his assumption that units of heat and of cold only mix, but do not destroy each other, he would have added the units of heat and of cold contained in the two samples of water, namely 2 + 4 of heat and 6 + 4 of cold, and then he would have obtained the proportion of 3 units of heat to 5 units of cold for their mixture. This would have been correct, at least as regards the degrees of heat. By a quaint exercise of ingenuity he missed the truth. He had, however, the merit of attracting the attention of Krafft to the problem.

KRAFFT

Georg Wolffgang Krafft (1701–54), Professor of Mathematics, then of Physics, at St. Petersburg, and later Professor at Tübingen, carried out various experiments on heat, and attempted to generalize Morin's formula for the resulting temperature of mixtures of water at different temperatures. He dropped all reference to units of cold, and confined himself to degrees of heat as measured on Fahrenheit's thermometer. The general formula which he suggested, when translated into more modern notation, was the following:

$$\theta = \frac{\alpha t_1 + \beta t_2}{\gamma m_1 + \delta m_2} \qquad . \qquad . \qquad . \qquad \text{(A)}$$

where θ is the temperature of the mixture, m_1 and m_2 are the two quantities of water, t_1 and t_2 are their respective temperatures, and α, β, γ, δ, are coefficients to be determined. Now when $t_2 = t_1$, then $\theta = t_1$; and the above formula becomes $t_1 = \dfrac{\alpha t_1 + \beta t_1}{\gamma m_1 + \delta m_2}$, so

that $\beta = \gamma m_1 + \delta m_2 - \alpha$, and $\alpha = \gamma m_1 + \delta m_2 - \beta$. By substituting the value of β in the original formula we obtain

$$\theta = \frac{(\gamma m_1 + \delta m_2)t_2 - (t_2 - t_1)\alpha}{\gamma m_1 + \delta m_2} \quad . \quad . \quad \text{(B)}$$

Similarly by substituting the value of α in the original formula we obtain

$$\theta = \frac{(\gamma m_1 + \delta m_2)t_1 + (t_2 - t_1)\beta}{\gamma m_1 + \delta m_2} \quad . \quad . \quad \text{(B')}$$

Again, when the quantity of hot water (at t_2) is negligibly small in comparison with the cold water (at t_1), then $\theta = t_1$, and $\gamma m_1 = \alpha$; if vice versa, then $\theta = t_2$, and $\delta m_2 = \beta$, according to (B) and (B') respectively. Substituting these values for α and β in formula (A) we obtain the equation:

$$\theta = \frac{\gamma m_1 t_1 + \delta m_2 t_2}{\gamma m_1 + \delta m_2} \quad . \quad . \quad . \quad \text{(C)}$$

In order to determine the values of the coefficients γ and δ Krafft mixed equal quantities of water at 44° and 120° F. respectively. The temperature of the mixture was 76° F. In this case $m_1 = m_2$; $t_1 = 44$; $t_2 = 120$; $\theta = 76$. Hence, according to (C), $\gamma = 11$ and $\delta = 8$, and $\theta = \frac{11 m_1 t_1 + 8 m_2 t_2}{11 m_1 + 8 m_2}$. This formula he repeatedly confirmed by mixing varying quantities of water at different temperatures, and noting the temperature of the mixture. (See his *De Calore et Frigore Experimenta Varia* in *Comment. Acad. Sci. Imp. Petrop.*, 1744–6, Vol. XIV, p. 218.)

RICHMANN

Prior to the publication of Krafft's results some similar work had been done, but dropped, by Richmann. After reading Krafft's paper, Richmann resumed his work on the same problem. Georg Wilhelm Richmann (1711–53) was Professor of Experimental Philosophy at St. Petersburg, where he was killed during a severe thunderstorm by a discharge from his own apparatus for measuring atmospheric electricity.

Richmann set out from the explicit assumption that the heat of a substance, or at least of a liquid, is evenly diffused throughout the substance. The intensity of a given quantity of heat will consequently vary inversely with the mass of the substance which it suffuses. Hence if the heat of a liquid of mass m_1 has an intensity t_1, and it is made to suffuse an additional liquid of mass m_2, then

the temperature of the mixture must be $\dfrac{m_1 t_1}{m_1 + m_2}$, assuming that the additional liquid had no heat of its own. Next, suppose conversely that the additional mass (m_2) has a temperature of its own, say t_2, while m_1 has no heat of its own, then the resulting temperature would be $\dfrac{m_2 t_2}{m_1 + m_2}$. But if m_1 has a temperature t_1, and m_2 has a temperature t_2, then by analogy the resulting temperature of their mixture, θ, should be $\dfrac{m_1 t_1 + m_2 t_2}{m_1 + m_2}$. In this way Richmann arrived at his formula (which is here restated in more modern notation):

$$\theta = \frac{m_1 t_1 + m_2 t_2}{m_1 + m_2}$$

The formula, as Richmann believed, is formally capable of extension so as to cover the mixture of any number of samples of liquid varying in mass and temperature. In its most comprehensive form it may be stated thus:

$$\theta = \frac{m_1 t_1 + m_2 t_2 + m_3 t_3 + \ldots m_n t_n}{m_1 + m_2 + m_3 + \ldots m_n}$$

It will have been noticed that Richmann's formula was the result of abstract *a priori* considerations, not the fruit of empirical observation or experiment. It was little more than a tentative hypothesis awaiting verification. Richmann realized this more or less, and tried to obtain empirical verification. At first he did so by reference to the published account of Krafft's experiments, and argued that Krafft's formula did not fit the observed results so well as did his own formula, when suitable allowance was made for the heat absorbed (or, maybe, imparted) by the vessel, the thermometer, and the air. Later, however, he carried out a number of experiments which he reported to the Imperial Academy of Sciences at St. Petersburg. Again he maintained that the results obtained conformed to his own formula more closely than to Krafft's formula, when due allowance was made for the heat absorbed by the vessel in which the samples of water were mixed, and by the thermometer with which the resulting temperature was measured. In making these allowances, however, Richmann did not try to assess them independently, but deduced them by comparing the actual temperature observed with the temperature which the mixture should have had in accordance with his own formula, if the vessel and the thermometer had not affected the result. And he had no suspicion that this method of procedure was really question-begging.

(See Richmann's memoir on "Formulae," etc., in *Nov. Comment. Acad. Sci. Imp. Petrop.*, 1747–48, Vol. I, pp. 168 ff.).

WILCKE

Johan Carl Wilcke (1732–96) was Professor of Experimental Physics at the Stockholm Military Academy, and, during the closing decade of his life, Secretary of the Swedish Academy. He was familiar with the work of Richmann, and accepted the latter's formula for determining the temperature of a mixture of liquids. Wilcke took up the experimental study of heat as the result of a happy accident. Early in 1772 Stockholm experienced a heavy snowstorm. Wilcke tried to melt some of the snow that was lying in his courtyard. He anticipated that hot water would melt a quantity of snow greatly exceeding the hot water in weight. Much to his surprise that was not the case, and he suspected that Richmann's law relating to the mixture of liquids did not apply to the mixture of water and snow. He investigated the problem experimentally, and found, for instance, that whereas when ice-cold water was mixed with an equal quantity of water at 68° C. the temperature of the mixture was 34° C., yet when water at 68° C. was poured on an equal weight of snow, the water fell to 0° C., and some of the snow remained unmelted. Wilcke then proceeded to carry out a long and systematic series of experiments in order to discover the law relating to the temperature of a mixture of snow and water. (See *K. Svenska Vet. Akad. Handl.*, 1772, Vol. XXXIII, pp. 97 ff.)

First of all he found that when a given weight of snow was mixed with an equal weight of water at varying temperatures, the temperature of the mixture was on an average about 36° C. less than it would have been if water at 0° C. had been used instead of the snow. Next he observed that when the weight of water mixed with the snow was double its weight, then the average loss was about 24° C., as compared with what the temperature of the mixture would have been if water at 0° C. had been used instead of the snow. Similarly, when the weight of the water was three times that of the snow, the mean loss was about 18° C.; when there was four times as much water as snow, the average loss was about $14\frac{3}{10}°$ C.; when there was five times as much water as snow, the mean loss was $12\frac{1}{5}°$ C.; when there was six times as much water as snow, the average loss was $10\frac{2}{8}°$ C.; and so on. Wilcke soon perceived that the numbers measuring the loss of temperature in the above mixtures were all of them fractions of 72; and he concluded that 72° of heat are required just to melt the snow, and that only the heat over and above the said 72° helps to raise the temperature of the ice-cold water into which it has been converted. In this way Wilcke discovered, independently of Black, the latent heat of the fusion of snow, though his estimate of it was not so good as Black's, and was arrived at about ten years later. It should be added that Wilcke also

discovered that the general formula for the temperature (θ) of a mixture containing a given weight of snow (n) and a given weight of water (m) at a given temperature (t) is as follows:

$$\theta = \frac{mt - 72n}{m + n}$$

After the discovery that snow when melting absorbs 72° C. of heat without showing any rise in temperature, the correlative thought naturally occurred to Wilcke that when water freezes it surrenders 72° C. of heat, and that supposing that only some portion of a mass of ice-cold water were to freeze, it would impart to the rest 72° C. of heat, while the ice formed would betray no loss of temperature as compared with its previous state as ice-cold water. But he did not succeed in experimentally verifying this idea. Nor did he appreciate the significance of his failure to do so, namely, that a colder body does not impart its heat to a warmer body. With a little more acuteness Wilcke might have been put on the scent of the Second Law of Thermodynamics.

Having discovered that when snow is converted into ice-cold water it absorbs heat which cannot be directly detected by means of a thermometer, Wilcke felt able to attack another problem which was troubling his contemporaries, namely, the problem of *specific heat*. It had been observed that when two substances (say, gold and tin) of the same volume and temperature were immersed in equal quantities of water having the same lower temperature, then the increase in the temperature of the water varied directly with the density of the immersed substance. It would seem, therefore, that the more dense substance contains more heat than does the less dense substance, though in a form that is not directly detectable by means of a thermometer. After trying many experiments with a great variety of substances, Wilcke concluded that the heat of different substances is not generally proportionate either to their volume or to their density alone, but that each kind of substance absorbs, retains, and imparts a certain quantity of heat, which in comparison with that of water or some other standard substance, may be called its *specific heat*. He explains, further, that the specific heat of any kind of substance might be described as the amount of heat contained in one particle of it, as compared with the heat contained in one particle of water (or some other substance used as a standard of comparison) of the same temperature. His conception of specific heat being more or less analogous to that of latent heat, Wilcke naturally sought to utilize the knowledge which he had acquired in the study of the temperatures of mixtures of snow and water in order to determine the specific heat of various substances. He tried various

methods. The simplest of them was based on the discovery that it was possible to ascertain the amount of heat which a body contained by ascertaining the amount of snow required to cool it from a given temperature to freezing-point, and that the quantity of snow required for this purpose could be determined indirectly. The method may be briefly described as follows. The substance (say, a piece of gold), the specific heat of which was to be determined, was heated to a definite temperature, and then immersed in an equal weight of water at 0° C., and the resulting temperature was noted. Richmann's formula (see p. 202) was then used to calculate what quantity of water at the temperature of the substance in question would give a mixture of the observed temperature when added to an equal mass of water at 0° C. Finally Wilcke's own formula (see p. 204) was made use of in order to calculate how much snow would be necessary to reduce the temperature of the said mixture to 0° C. In this way Wilcke ascertained the specific heat of the following substances, using the specific heat of water as his unit: gold (0·050), lead (0·042), silver (0·082), bismuth (0·043), copper (0·114), iron (0·126), tin (0·060), zinc (0·102), antimony (0·063), glass (0·187). (See *K. Svenska Vet. Akad. Nya Handl.*, 1781, Vol. II, pp. 49 ff.) Black's work on specific heat anticipated Wilcke's by some twenty years, and Wilcke had some slight knowledge of it at second-hand. Before concluding this section a few words may be added concerning Gadolin's contribution to this branch of science.

GADOLIN

Johan Gadolin (1760–1852) was born in Finland, studied under Bergman, and was a friend of Scheele. He was a distinguished chemist; he discovered one of the rare earths, yttria; and the rare metal, gadolinium, was named in his honour. Here, however, we are only concerned with his work on heat. He appears to have been one of the first, if not the first, to introduce a formula for the temperature of a mixture, in which due allowance was made for specific heat. Setting out from Richmann's formula—$\theta = \dfrac{m_1 t_1 + m_2 t_2}{m_1 + m_2}$ —Gadolin pointed out that when the substances mixed are different in kind, then the resulting temperature of the mixture cannot be correctly calculated from this formula; allowance must be made for the differences in their specific heats. Gadolin accordingly gave the following formula:

$$s_1 : s_2 :: m_2(\theta - t_2) : m_1(t_1 - \theta),$$

where s_1 and s_2 represent the specific heats of the two substances mixed, while the remaining symbols have the same meaning as in

Richmann's formula. Gadolin's formula can, of course, be extended so as to embrace mixtures of more than two different kinds of substances. Moreover, unlike Richmann, Gadolin made a serious and fairly successful attempt to arrive at a correct and independent estimate of the allowance which has to be made, when dealing with the temperature of a mixture, for the heat which the mixture imparts to the vessel containing it; and he produced a formula for estimating the mass of the liquid which would affect the temperature of the mixture to the same extent as is done by the containing vessel. His formula for this is, $\dfrac{m(t - \theta)}{\theta - t_v}$, where m represents the mass of the liquid mixture, t its temperature, θ the temperature of the liquid and the vessel containing it, and t_v the temperature of the vessel without the liquid. (J. Gadolin and N. Maconi: *Dissertatio chemico-physica de Theoria Caloris Corporum Specifici*, Åbo, 1784.)

J. INVISIBLE RADIANT HEAT

The study of invisible radiant heat, the beginnings of which go back to the seventeenth century, made no further progress for a period of about eighty years after 1682. These intervening years, however, were indirectly preparatory to further research on the subject. For during these decades burning mirrors and lenses were vastly improved, and great progress was made in the construction and graduation of thermometers. These improved instruments were a great aid in this field of research. Hence the advances made in the latter half of the eighteenth century, when work on these problems was resumed.

Wolfe (*Phil. Trans.* 1769, p. 4) described some parabolic burning mirrors, and certain experiments carried out by Hoffmann with them. In one experiment, the heat of a fire was concentrated at the focus of a mirror. In another, glowing coals were placed at the focus of one mirror and, using two specula, combustibles were ignited at the focus of another mirror. The experiments were repeated, using a strongly heated stove, and again combustibles were kindled. Thomas Young, in his *Lectures on Natural Philosophy*, 1807 (I, p. 637), said that Hoffmann was the first to collect the invisible heat from a stove in this way, i.e., by reflection from one or more mirrors. Buffon (*Histoire Naturelle, Supplément*, 1774, I, p. 146) gave a more satisfactory proof than Hoffmann. "I received on a burning mirror a fairly strong heat *without any light*, by means of a plate of iron placed between the bright fire and the mirror; part of the heat

was reflected to the focus of the mirror, while all the rest of the heat penetrated it." (Hoffmann does not appear to have taken any precautions to eliminate all traces of light from the source he used — a stove.) In the same work Buffon wrote: " It appears . . . that two sorts of heat must be recognized, the one luminous . . . and the other obscure." His experiment proved that bodies possessing invisible heat emitted invisible rays capable of reflection like rays of light.

Scheele was the first to make systematic experiments in this field. He tried to harmonize theory and experiment, and formulated a theory of radiant heat and light that fitted the available facts and did not run counter to the prevailing phlogiston theory. In his *Chemical Treatise on Air and Fire* (Upsala, 1777, English translation, L. Dobbin, London, 1931, p. 120), he refers to the experiment in which the heat of "brightly red-hot charcoal" placed in the focus of a concave metal mirror may be collected at the focus of another similar mirror where combustibles may then be ignited; and he asks whether this effect is due to the heat or the light of the charcoal, or to both. In his experiments on the problem thus stated, he distinguished carefully between radiant heat and the heat due to convection currents—and studied the former. As source, Scheele used an open stove. The heat radiated from it was not affected by a strong draught over the mouth of the stove. He then put a large glass plate between his face and the stove—and he could not feel any heat. Then he studied the reflection of the rays by plane and concave mirrors of silvered glass and of metal. With silvered glass he found that the light was reflected and that the heat was absorbed by the glass. With metal mirrors, both heat and light followed the same laws of reflection as the sun's rays. After the rays from the stove had passed through a glass plate they produced no heat even when concentrated by means of a lens or a mirror.

Scheele produced a focal point that kindled sulphur by means of a concave metal mirror held at a distance of two ells in front of the stove. He noted also that the mirror did not grow warm, but if it was blackened with soot by holding it over a burning candle and then held in this position in front of the stove, it burned his fingers in a few minutes. Metal mirrors and plates grew hot in contact with hot bodies—but not from the heat of the stove. When the uppermost flue of the stove was closed, the heated air escaped upwards from the open door of the stove, and if the concave metal mirror, or the metal plate, was held in this ascending heat, this heat was not reflected but the metals became hot.

Hence Scheele inferred certain properties of radiant heat. "It follows," he wrote, "from these experiments that the heat rising

along with the air in the stove and passing through the flue is really different from that proceeding through the stove door into the room: that it travels from the place of its generation in straight lines and is reflected again by polished metals at the same angle as the angle of incidence: that it does not unite with the air, and consequently cannot acquire from the current of air any other direction than it received at the beginning of its generation" (*ibid.*, p. 123). "These," added Scheele, "are properties which belong to light." But he would not point to light as the cause of the phenomena, because (1) the light of the fire was far too weak, as compared with that of the sun; (2) the heat of a focal point tested by the kindling of sulphur was greater when the wood was consumed and burned to "brightly red-hot charcoal" and, therefore, giving less light; and (3) the heat and light of the fire were separable by a glass mirror which reflected the light and retained the heat.

Scheele therefore concluded that radiant heat had some of the properties of light, and that it had not yet become light, since it underwent a different kind of reflection from a glass surface than from a metal surface—"a remarkable circumstance!" he wrote (*ibid.*, p. 123). Then he named this kind of heat "radiant heat," and in terms of this answered his original query as to why the rays reflected from red-hot charcoal ignited combustibles, by stating that it was due to "the radiant heat which is invisible and differs from fire" (*ibid.*, p. 125).

In the course of further experiments on light, Scheele claimed that he had proved that the igniting power of radiant heat was not due to the light in it; but this was only true for the radiant heat from a fire, not for that from the sun's rays, the heat and light of which passed equally well through glass, as Mariotte had shown. He concluded that radiant heat was a material substance, a compound of fire, air, and phlogiston, while light was a similar compound containing more phlogiston.

Thus Scheele had introduced the term "radiant heat," described the main properties of this kind of heat, and distinguished it from light. His experiments were much more valuable than his theoretical speculations, which were often false. For instance, he argued that the colours violet and purple contained less phlogiston, as they were more attracted by the prism (i.e., they suffered greater refraction); therefore, since radiant heat had still less phlogiston, it would be still more attracted. Invisible heat rays would therefore be found, it seems, beyond the violet end of the spectrum. He could not have tested this experimentally.

J. H. Lambert (*Pyrometrie*, 1779, § 378) gave two proofs that the heat of a fire existed, not as light, but as obscure heat: (1) because

transparent glass protects the face from the heat of the strongest fire until it grows hot itself, and (2) because the image of the strongest fire focussed by a burning lens on the hand does not give rise to the slightest sensation of heat. Thus glass and other transparent bodies separated the light from the heat of a fire—transmitting the light and absorbing the heat. While the heat of a fire could not be concentrated by lenses, it could be concentrated by mirrors. (Some say that this had been shown by Zahn at Vienna as long ago as 1685, but no evidence seems traceable.) Lambert repeated these experiments successfully. The rays from a charcoal fire, placed at the focus of a concave mirror (18 inches focus) were collected by a smaller concave mirror (9 inches focus) placed opposite the first mirror at a distance of 20 to 24 feet; and tinder, etc., placed in the focus were ignited. These mirrors were evidently metallic. Lambert said that this result was entirely due to the obscure heat, although light was also concentrated at the focus. His experiment supported Scheele's conclusions.

Kries (*Annales de Chimie et de physique*, 1809, 71, 158) refers to a work called *An account of parabolic wooden mirrors and their surprising action, newly invented by André Gaertner* (1785), in which an experiment was described which demonstrated the reflection of the obscure heat from a heated iron stove. When its heat was concentrated by the parabolic mirror at a distance of from ten to twelve paces, it was equal to that of an open fire concentrated at twice this distance. Gaertner also described how by placing ice at the focus of his mirror there was a very considerable production of cold at ten to twelve paces distance.

De Saussure (*Voyages dans les Alpes*, 1786, II, pp. 353 f.) considered that Lambert's experiments were not decisive—the source ought to be non-luminous. So he repeated the work, using an iron bullet, hot but not red-hot, and experimented jointly with Pictet on the latter's apparatus. Two tin concave mirrors, of the same size and focus, were placed opposite each other 12 feet 2 inches apart. The iron bullet was first made red-hot, and when cooled until it was invisible in the dark it was placed in the focus of the first mirror. In the focus of the other mirror was placed the bulb of a mercury thermometer, which thereupon indicated a rise of 8° above the temperature produced by direct rays alone, the latter being indicated by a thermometer placed just outside the focus. There was no other source of heat in the room where the experiment was made; and the same result was obtained on various days with various thermometers. The least shifting of the thermometer from the focus produced a big fall in temperature—almost to room temperature. These experimenters were thus satisfied as to the reflection of obscure heat. As

to the nature of this obscure heat, de Saussure considered it to
consist in calorific oscillations produced by the agitation of the heat
fluid in bodies, oscillations that could be reflected as sound-waves
are reflected; and he suggested an experiment to measure their speed.
The experiment was carried out later by Pictet, as will be explained
presently.

De Luc (*Idées sur la Météorologie*, 1786–87) held that the reflection
of heat was proved by the fact that metal pots containing water took
longer to boil when polished outside than when left covered with
soot, since the particles of fire were reflected from the polished
surfaces according to the law of reflection for all rebounding bodies
(angle of incidence = angle of reflection). He thought also that the
sun's rays were not calorific in themselves, but became so by
combining with a substance present in the atmosphere, which
substance deprived them of their property of giving light; and he
consequently opposed Du Carla, who, in his *Feu Complet*, had
assigned the light-giving power and the heat-giving power of the
solar rays to a common agent.

In 1788, Edward King (*Morsels of Criticism*, I, 99) described
experiments in which the invisible heat from boiling water was
reflected by concave mirrors and refracted to a focus by a convex
lens; but it is doubtful whether from the small effects observable
in his experiments he was justified in concluding that the fluid of
heat had the same reflexibility and refrangibility as the rays of light.
In later experiments, with concave metal mirrors, he reflected the
heat of a fire, and noted that "metal is much more efficacious for
this purpose than glass."

In 1790, M. A. Pictet (*Essais de Physique*, English trans., *An Essay
on Fire*, by W. B., 1791) stated that "Liberated Fire" (i.e., radiant
heat) "is an invisible emanation which moves according to certain
laws and with a certain velocity" (p. 8 of Eng. trans.). He compared
it with light, and, since heat could be obtained without light, and
vice versa, concluded that they were related as a whole to a part.
As to the nature of this "Liberated Fire," he preferred, on account of
its simplicity, the theory that it was a real emanation to the view
that it consisted of simple vibrations in the perfectly elastic all-
pervading heat fluid. He repeated the experiment (on the reflection
of obscure heat) which he had previously made with De Saussure (see
above), using a hot iron ball, and showed that a wax taper produced
an equal effect, but that a glass plate cut off about two-thirds of the
heat. As a further proof of reflection of invisible heat, Pictet repeated
the experiment with a small flask of boiling water, which should
give a satisfactory non-luminous source. With the mirrors 10 feet
6 inches apart, the rise in the mercury thermometer was $3\frac{1}{8}°$ F. in

two minutes, and, as soon as the flask was removed from the focus, the temperature fell. He showed that pure heat, like light, was absorbed by black bodies, for when he blackened the thermometer bulb, the rise in temperature was $4\frac{1}{2}°$ F., and occurred more quickly. When a glass plate was placed between the mirrors, most of the heat was absorbed.

Since heat was reflected according to the same law as light, Pictet tested for refraction by reflecting the heat from the flask of boiling water by means of a concave tin mirror on to a convex lens with a thermometer at its focus. He used three different lenses, but could find no more heat at their foci than elsewhere, and the question remained undecided. He also attempted to measure the velocity of radiant heat by the method suggested by De Saussure. The hot iron ball, invisible in the dark, was placed at the focus of the first tin mirror and shielded from the other mirror by a thick screen. At a distance of 69 feet, a larger gilt mirror was placed opposite the first mirror, with a sensitive air thermometer at its focus. When this was steady the screen was removed, the thermometer rose at once without any appreciable time interval. Pictet therefore concluded that "Liberated Fire," as he termed it, moves "in straight lines and in every direction with a considerable velocity, perhaps as rapidly as sound or even light"; and here too he named it "radiant heat" (Eng. trans., p. 113).

Pictet tried also the experiment on the reflection of cold. The tin mirrors were placed $10\frac{1}{2}$ feet apart; a flask filled with snow was set in the focus of the first mirror, and the bulb of a sensitive air thermometer in the focus of the second. At once a fall of several degrees occurred, but the thermometer rose again when the flask was removed from the focus. A still greater effect was produced when nitric acid was poured on the snow (to get a lower temperature). Pictet's final explanation of these facts followed Prevost's theory, to which we now turn.

In 1791, Prevost published his theory of the equilibrium of radiant heat by continual exchanges (*Observations sur la physique*, 1791, 38, 314). This was based on De Luc's theory of heat (as a discrete fluid, the particles of which are in constant motion) and on Pictet's demonstration of the apparent reflection of cold. In fact, the theory was devised mainly to explain this puzzling experiment. Prevost started by comparing light and radiant heat ("perfectly free fire"), and concluded that the experimental evidence, though limited, justified the conclusion that they were similar in properties, especially in rectilinear and instantaneous propagation. Hence, for him, heat and light were similar discrete fluids. Taking the case of two adjoining parts of space at the same temperature, he maintained that there

were continual exchanges of heat between them by radiation. These exchanges were equal, and hence the two parts were in equilibrium relatively to one another, and their temperature remained constant. Now, if one part got hotter, it would give out more heat than it received from the other, and this would go on until a new state of equilibrium was reached at a higher temperature.

This theory gave a very satisfactory explanation of the apparent reflection of cold; for a cold body at one focus would emit less radiant heat than a warmer body at the other focus, which would be cooled because it would receive less radiant heat than it emitted. Thus the effect on the thermometer was due, not to the reflection of cold, but to the reflection of heat in the reverse direction. Pictet at once accepted this explanation.

James Hutton (*Dissertation on the Philosophy of Light, Heat and Fire,* Edinburgh, 1794) repeated Scheele's experiment on the effect of a sheet of glass on the rays from a fire, and found that the heat was not entirely absorbed but merely diminished in intensity. He denounced the notion of "obscure heat" and said that "to suppose heat moving without body, or reflected according to the laws of light, is certainly an idea that would disgrace science." To him, the effects were rather due to invisible light which, too weak to affect the eye, was yet strong enough to communicate heat.

Thus by 1800 the existence of invisible heat rays was generally accepted. It was known that these rays were propagated almost instantaneously in straight lines, and were reflected according to the same laws as light rays. It was suspected that they could also be refracted, like light rays. These similarities with light led to a suspicion that the two were connected. Hutton even argued that the invisible heat rays were really invisible light. But since glass exhibited different absorption effects on the rays of heat and light from a fire, most scientists reserved judgment, and used the terms *obscure heat* or *radiant heat* for the invisible heat rays. Of the two proposed theories, namely, (*a*) that the invisible heat rays were a material emanation, and (*b*) that they were vibrations in an all-pervading heat fluid, the former was more generally held. The reflection of cold had now been demonstrated and explained. Experiments had been mainly qualitative, but the field was now prepared for quantitative investigation.

(See D. McKie and N. H. de V. Heathcote, *The Discovery of Specific and Latent Heats,* London, 1935; E. Mach, *Prinzipien der Wämelehre,* Leipzig, 1923; E. S. Cornell, "Early Studies in Radiant Heat," *Annals of Science,* 1936, Vol. I, p. 217; and the general books on Physics mentioned on p. 172.)

PHYSICS

IV. ELECTRICITY & MAGNETISM (I)

WHILE in the seventeenth century the greatest advances in Physics had occurred in the departments of mechanics and optics, which were among the oldest branches of natural science, the eighteenth century was remarkable for its developments in the realm of frictional electricity which had been opened up by Gilbert and Von Guericke. Progress in the study of frictional electricity was, however, necessarily very slow at first, since it was dependent merely upon chance observations unco-ordinated by any theory. Every exact science has to go through this earliest stage, but electrical science was the last of all the principal branches of Physics to outgrow it. It was not until well into the eighteenth century that it entered upon the second stage, which is characterized by systematic experimentation directed by hypothetical conceptions. The earlier stage is represented by such men as Hauksbee and Du Fay, whose periods of activity fell at the beginning of the eighteenth century. Franklin and Aepinus, who stood upon the shoulders of their predecessors, belong to the second period. But it was reserved for the close of the eighteenth century to arrive at the precise laws of frictional electricity through quantitative observation. This was the achievement of Coulomb, upon whose experimental researches were based the subsequent mathematical deductions which finally made electrostatics an exact science.

A. FRICTIONAL ELECTRICITY

HAUKSBEE

Electrical researches at the beginning of the eighteenth century were especially stimulated by interest in the remarkable phenomenon of mercurial phosphorescence which had been discovered by Picard in 1675. Upon shaking the mercury column of a barometer in the dark a peculiar phosphorescence may be observed in the Torricellian vacuum. This singular phenomenon created a considerable sensation, and much was written about it. There were controversies as to its nature, in which Johann Bernoulli took a considerable part. The effect was at first generally attributed to the presence of sulphur or of a special "phosphorus" in the mercury, but the correct explanation was eventually given by Francis Hauksbee (*d.* 1713 ?), Fellow and Curator of the Royal Society. By his experiments Hauksbee correctly

established that the phenomenon is due to the generation of electricity by the friction of the mercury on the sides of the glass tube. He contrived a number of independent experimental proofs of this hypothesis, and he showed that luminosity appears even when air is present at normal pressure above the mercury surface. Hauksbee's explanation was confirmed when in 1745 Ludolff of Berlin showed that when mercury was agitated in a barometer-tube some threads hanging in an exhausted container surrounding the tube were attracted thereto (*Mém. de l'Acad. de Berlin*, 1745).

Illustr. 95.—Hauksbee's Electrical Machine

Hauksbee, however, went beyond this limited problem in the course of the researches which he described in the *Philosophical Transactions* from 1705 onwards, and in his book *Physico-Mechanical Experiments on Various Subjects, etc.* (London, 1709).

He contrived a machine for rotating objects rapidly in the exhausted receiver of an air-pump. By this means he rubbed amber on wool in the partial vacuum of the receiver, and observed luminosity at the points of friction which remained visible so long as the motion was maintained. Later he rotated a glass vessel in the receiver under friction with a woollen pad, when a "fine purple Light" was produced. In both cases the luminosity was appreciably diminished by the admission of air, and was most noticeable every time fresh materials were used. The effects of friction *in vacuo* between several other pairs of substances (glass on glass, etc.) was also noted. In some further experiments which he showed to the Royal Society, Hauksbee employed a rudimentary form of the glass electrical machine. The earliest mechanism for the generation of electricity by friction was that constructed by Von Guericke about the middle of the seventeenth century, and Hauksbee was probably acquainted

with Guericke's description of it. This machine did not come into general use, however; and until the early years of the eighteenth century at least, the practice continued of generating electric charges merely by rubbing pieces of glass, amber, or other electrical substances with the bare hand or with suitable materials. Hauksbee's machine consisted of a hollow glass globe about nine inches in diameter, which was exhausted, sealed, and rotated rapidly about an axle (Illustr. 95). Upon applying the bare hand to the rotating globe, sparks an inch long were obtained, and enough light was produced to read by. In a subsequent form of the instrument the glass was lined with a thin layer of sealing-wax, pitch or sulphur, when the rubbing hand appeared as a luminous trace upon the inner surface of the lining. Hauksbee attributed the effect to the discharge of humid effluvia (in terms of which he explained all these phenomena) previously condensed on the glass, and he described the transforma-tions which he observed upon gradually readmitting air to the globe. Here again he recognized the similarity and probable analogy of this luminosity to that observed in an agitated barometer.

Hauksbee may thus be regarded as the inventor of the glass electrical machine, which, however, did not come into immediate general use, and whose development must be considered later. He further experimented with the construction of electrical machines operated by the rotation of cylinders of sealing-wax, sulphur, resin, etc., though without discovering that there are two kinds of elec-tricity. His further papers on electricity deal with the transmission of electrical influence through glass, and with the production of luminosity in exhausted globes in the mere neighbourhood of elec-trically excited glass. His investigation of the properties of magnetic force is treated elsewhere. Though Hauksbee himself made no discoveries of fundamental importance, his work attracted attention to electrical phenomena and stimulated their further investigation.

GRAY

The earliest observations of the conduction of electricity seem to have been made by Von Guericke. He did not follow the matter up, however, and it was left for Stephen Gray (d. 1736), a pensioner of the Charterhouse and a prolific experimenter of whom but little is known, to describe explicitly the phenomenon of conduction, and to distinguish experimentally between conducting and non-conducting substances.

Gray, in 1729, took a glass tube and corked it at each end, in order to see whether under these conditions it could still be electrified by rubbing. He noticed incidentally that the corks as well as the tube became electrified, and would attract a feather. He next took

a wooden rod and thrust one end of it into one of the corks, and the other end into a hole through an ivory ball. He found that when the tube was excited the ball attracted the feather, the influence having been transmitted to it through the cork and the wood. In order to ascertain to what distance such transmission was possible, Gray tried longer and longer wooden rods, and finally wires or lines of packthread, to connect the excited tube with the ball. He found that the attractive power continued to be communicated to the latter, and there seemed to be no limit to the distance over which the influence would travel. Gray and his colleague, Granvil Wheler, found that the experiment broke down when they used packthread to support the conducting lines, but that it succeeded again when silk was used. At first they thought that the silk threads, being smaller than the packthread, carried away less effluvia, but when they used fine brass wire and again failed, they were convinced "that the Success we had before, depended upon the Lines, that supported the Line of Communication, being Silk, and not upon their being small"; and that "when the Effluvia come to the Wire or Packthread that supports the Line, it passes by them to the Timber, to which each End of them is fixed, and so goes no farther forward in the Line that is to carry it to the Ivory Ball" (*Phil. Trans.*, 1731, p. 81). The greatest distance through which the influence was transmitted was 765 feet. By Gray's experiments a clear distinction was indicated between those bodies which would convey electrical properties to other bodies at a distance, and those bodies (including hair, silk, resin, and glass) which would not do so, and could therefore be employed for the preservation of charges. Gray anticipated the discovery of electrical induction by showing that the electric virtue could be conveyed from the excited tube by merely holding it near the conducting thread.

Among numerous other experiments described by Gray, only a few can be mentioned here. He took two oaken cubes of equal size, one solid and the other hollow, suspended them by hair-cords, electrified them inductively by connecting them by a conducting line of communication and placing a rubbed glass tube over the middle of this line so as to be at equal distances from the two cubes. Equal attractions were then observed in pieces of leaf-brass placed at equal distances below the two cubes. Gray and Wheler electrified objects of all descriptions, including a boy, a cock, a sirloin of beef, a red-hot poker, an "umbrello," and a map of the world, having previously suspended them by silken cords. At other times Gray placed persons or objects upon a cake of resin before charging them, and is thus essentially the inventor of the insulating stool. He placed a small vessel full of water upon such an insulator, and, upon bringing a

charged glass rod near the surface, he noticed that the water in its vicinity rose above the level of the rest. Gray also confirmed Hauksbee's discovery that the electric influence can act through glass, and he observed the luminous discharge from a pointed iron rod suspended near the excited glass tube.

DESAGULIERS

Gray's discovery threw light upon Gilbert's distinction between *electric* and *non-electric* substances, the latter class simply comprising those bodies from which any charge was carried away as rapidly as it was generated. In some experiments which he performed in continuation of those of Gray, Jean Théophile Desaguliers (1683–1744) applied the name of *conductors* to those substances through which electricity could be transmitted, and that of *electrics per se* or *supporters* to those which lacked this property, and which therefore served to support the conducting threads which he employed in his experiments (*Phil. Trans.*, 1739, p. 193). In the latter class of substances electricity may be excited by actions upon the body itself, while the former class cannot be charged by any immediate action upon them, but can only *receive* electricity from an electric *per se*. He recognized, however, that an electric *per se* could readily be converted into a conductor by moistening it with water. Desaguliers was also a great expositor and popularizer of the new science.

DU FAY

The true nature of the distinction between electric and non-electric substances was recognized about the same time by several other investigators. These included Charles François Du Fay (1698–1739), of Paris, who performed numerous electrical experiments, some of which are of fundamental importance, and who confirmed Gray's results. His principal discoveries, which were described in the *Mémoires* of the French Academy of Sciences (1733–37) and in the *Philosophical Transactions*, may be summarized in the following terms: (1) An electric body when charged attracts all non-electric bodies, communicates electricity to them, and thereupon repels them. (2) There are two opposite kinds of electricity, *vitreous* electricity and *resinous* electricity. This second most important discovery was made known to English readers by a letter from Du Fay published in the *Philosophical Transactions* for 1734, where it is formulated in the following terms:

"Chance has thrown in my way another Principle, more universal and remarkable . . . and which casts a new Light on the Subject of Electricity. This Principle is, that there are two distinct Electricities, very different from one another; one of which I call *vitreous Electricity*

and the other *resinous Electricity*. The first is that of Glass, Rock-Crystal, Precious Stones, Hair of Animals, Wool, and many other Bodies. The second is that of Amber, Copal, Gum-Lack, Silk, Thread, Paper, and a vast Number of other Substances. The Characteristick of these two Electricities is, that a Body of the *vitreous Electricity*, for example, repels all such as are of the same Electricity; and on the contrary, attracts all those of the resinous Electricity" (*Phil. Trans.*, Vol. XXXVIII, p. 258).

Du Fay was led to this discovery (i.e., of the two kinds of electricity) by experiments which he reported to the Paris Academy (*Mém. Acad. Roy. Sci.*, 1733, p. 464). He was under the impression that a gold leaf which had been electrified by a rubbed glass rod would be repelled by every other body electrified by friction. This assumption, however, proved to be false, for when he brought rubbed resinous bodies up to the gold leaf, it was attracted by them. It was this experiment also which led Du Fay to name the two kinds of electricity as he did. Later, the experiments of Canton and of Wilcke showed that his names were misleading, since resinous bodies may develop vitreous electricity and vitreous bodies resinous electricity, when rubbed with suitable materials.

As already mentioned, Du Fay followed Gray in pointing out the connection between the capacity of bodies to conduct away electric charges, and their capacity to receive charges themselves. He showed that Gilbert's non-electrics could be electrified if they were supported or suspended by "electric" substances, which began to be used extensively at this time as insulators. Thus he succeeded in electrifying a person suspended by hair or silken cords, and in drawing sparks from him.

ELECTRICAL MACHINES

Following the work of Hauksbee, the electrical machine underwent a gradual evolution during the eighteenth century, when it greatly assisted the further investigation of the phenomena of frictional electricity. For about thirty years after Hauksbee's death, the possibilities of such machines were ignored, and the practice continued of generating charges by rubbing glass rods with pads of wool or leather to which powdered chalk or other such substances had been applied. A continuous tradition in the design and use of electrical machines was begun by C. A. Hausen, of Leipzig, in 1743. In his *Novi Profectus in Historia Electricitatis* of that year, he described such a machine, consisting of a glass globe which was rotated rapidly by means of a belt of cord passing round a large wheel, turned by a handle. In the figure illustrating Hausen's apparatus, a boy is
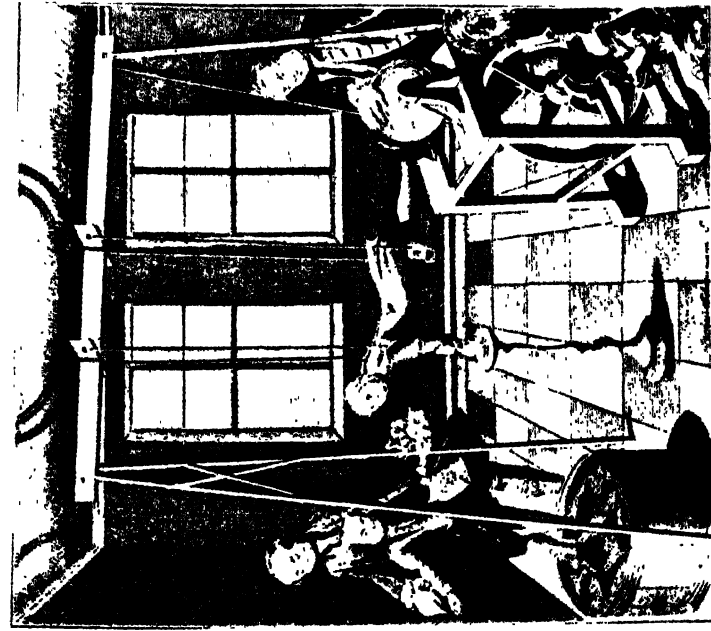
Hausen's Electrical Machine
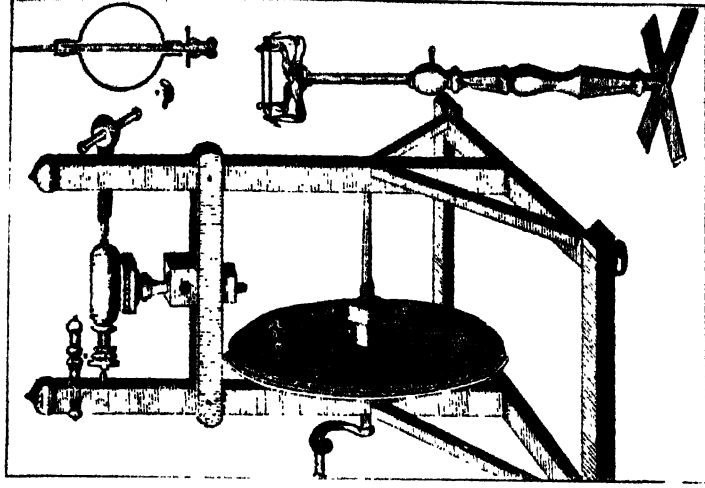
Gordon's Electrical Machine
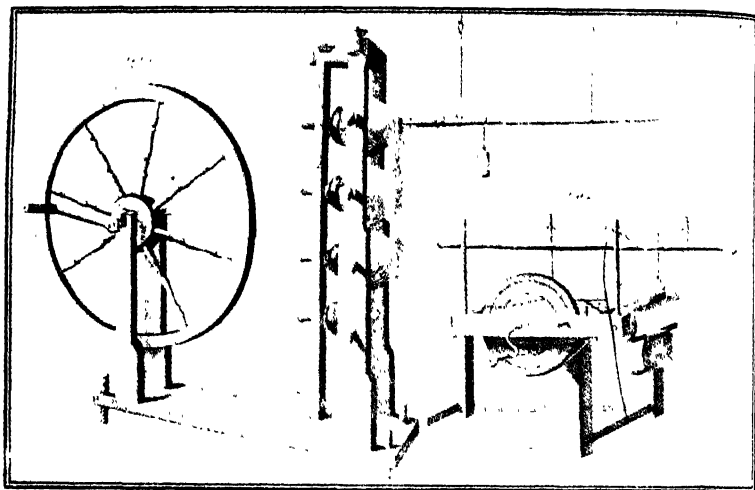
Watson's Electrical Machine   Wilson's Electrical Machine
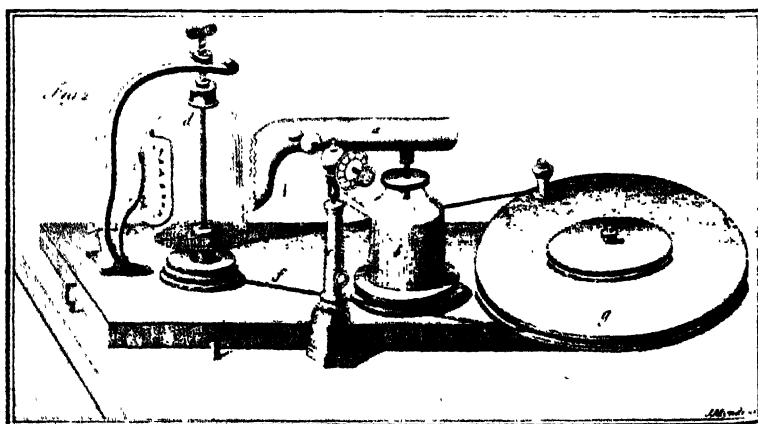
Read's Electrical Machine

shown hanging in silk cords, with his feet touching the whirling globe, so as to serve at once as a rubber and a sort of prime conductor from which sparks could be taken (Illustr. 96). Shortly afterwards G. M. Bose described his own improvements in a poem *Die Elektricität* (Wittenberg, 1744), and in *Tentamina electrica* (Wittenberg, 1744). Bose claims to have used a glass globe, for the purpose of generating charges, as early as 1737. He substituted, for insulated human beings, iron tubes hung on silk threads and placed in communication with the globe by means of linen threads. J. H. Winkler, the Leipzig electrician, constructed machines comprising a number of glass globes all working at once. On the advice of his fellow-townsman, Giessing, he employed mechanical rubbers consisting of leather-covered cushions treated with chalk and pressed against the globes with springs (*Gedanken von den Eigenschaften, Wirkungen und Ursachen der Elektricität, nebst einer Beschreibung zwo neuer elektrischen Maschinen*, Leipzig, 1744). The necessity for earthing such rubbers had to be recognized, after which they soon became established. But Winkler in the same book describes a machine in which a long glass tube was worked backwards and forwards by means of a treadle, so as to rub against a pad which enclosed it; and he also depicts a mechanism for whirling a drinking-glass about its axis, first one way and then the other, by means of a cord wrapped round the axis and worked by a treadle and spring, somewhat as a drill is worked with a bowstring. Winkler describes yet another machine in *Die Eigenschaften der elektrischen Materie*, etc., Leipzig, 1745, in which the prime conductors were built in as part of the machine. It was recognized by J. G. Krüger (*Zuschrift an seine Zuhörer*, Halle, 1745) that the longer the prime conductor was, the greater the strength of the shock which it would give when charged; it was left to Franklin to recognize that the shock depends (under given circumstances) upon the *area* of the conducting surface. A. Gordon, in his *Versuch einer Erklärung der Elektricität* (Erfurt, 1745) describes a machine of his own construction, in which a glass cylinder was rotated against a leather pad (Illustr. 97). The prime conductor was an iron tube which was separate from the rest of the machine; one end of this tube was brought up to within a quarter of an inch of the cylinder when the machine was in action, and it served to collect the charge. (See F. Rosenberger: *Die erste Entwicklung der Elektrisirmaschine*, in *Abhandlungen zur Geschichte der Mathematik*, Heft 8, No. 3, 1898.) Some of the subsequent advances in the design of electrical machines, in the middle years of the eighteenth century, are described and depicted by Joseph Priestley in his *History of Electricity* (3rd ed., London, 1775, Part V, Section II). The machines there noticed include Watson's, in which friction was applied to three or four globes rotating simultaneously (Illustr

98); Wilson's, in which a metal comb was first used to collect the charge from an electrified cylinder; Read's, in which the prime conductor with its toothed collector was joined to the interior of a Leyden jar; and another, in which a globe was turned at a great speed by means of gearing (Illustr. 100). A rather different type of machine, noticed by Priestley (Illustr. 101), seems to have been independently invented about 1766 by Ingenhousz and Ramsden, and perhaps also by Planta. It consisted essentially of a circular plate of glass; this was rotated in a vertical plane by a crank attached to an iron axle going through the middle of the plate; and it was rubbed by four cushions which were applied on each side of the plate at opposite ends of its vertical diameter. The prime conductor was a hollow brass tube from which two horizontal branches, armed with collecting-points, approached to within half an inch of the glass disc, each taking the electricity excited by one of the pairs of cushions. Priestley also gives an account of his own machine (Vol. II, pp. 112 f.), "the result of my best attention to this subject." It is shown in two forms, consisting essentially of a globular glass flask into the neck of which a metal axle was cemented, without, however, passing right through the globe (Illustr. 102). The rubber was pressed against the globe, which was rotated by turning the axle, and from which the charge was collected by pointed wires lightly sweeping the surface of the globe.

Canton showed how to produce stronger frictional charges by treating his oiled silk rubbers (which were applied to glass rods) with a mixture of mercury, tin, and chalk (*Phil. Trans.*, 1762, p. 457). The rubbers of electrical machines, too, were treated with various substances to increase their efficacy. Of these an amalgam of zinc, tin, and mercury, due to Von Kienmayer, gave the best results (*Journal de Physique*, 1788). Attempts were also made to improve the machine by lining the glass cylinder or globe with a resinous composition.

Nooth noticed that when an electrical machine was worked in the dark a luminous discharge occurred at the place where the revolving cylinder left the rubber, and he realized that much of the electricity generated returned to the cushion and never reached the collector at all. He therefore fitted a non-conducting flap, composed of many thicknesses of silk treated with bees-wax, to the side of the cushion where the cylinder left it. The silk adhered to the cylinder by attraction, and served as a screen to prevent electricity from returning to the cushion. Nooth also joined the other side of the cushion to the metal base upon which it rested, by means of a conducting flap, so as to facilitate the flow of electricity to the place where excitation of the cylinder occurred (*Phil. Trans.*, 1773, p. 333).
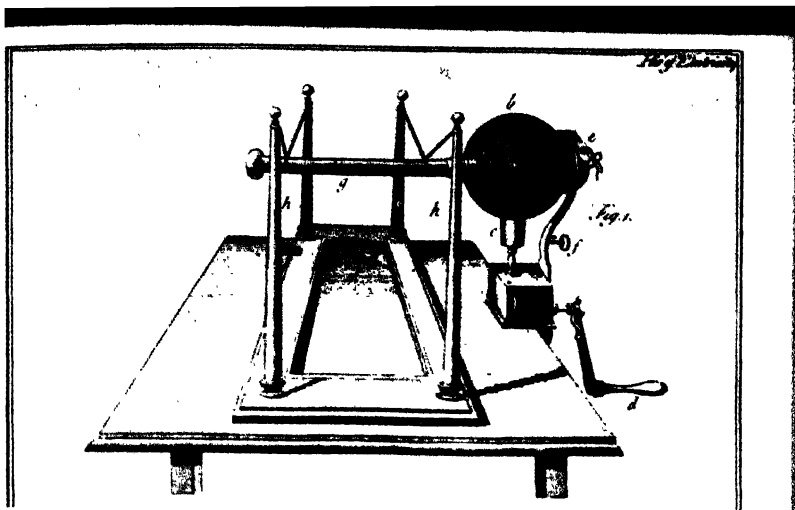
Illustr. 101   Anonymous Electrical Machine
Illustr. 102
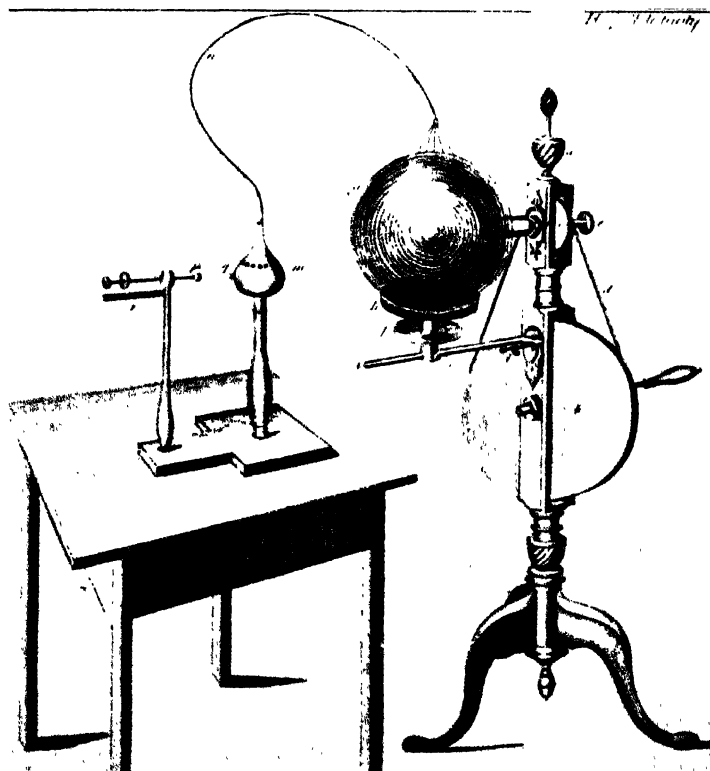
The electrical machine soon became very fashionable, and in the hands of wealthy amateurs it eventually assumed huge proportions. The principal remaining phenomena of frictional electricity were now discovered in rapid succession. The inflammatory action of the electric spark was proved on gunpowder, ether, spirit of wine, phosphorus, etc. Such an experiment is shown in Illustr. 103, where the charge is transmitted from the machine to a combustible substance through the insulated body of a man. In this manner Gralath, the Burgomaster of Danzig, rekindled with the electric spark a candle which he had just blown out, while it was shown that spirit of wine could even be ignited by means of an electrified jet of water (*Versuche u. Abhandl. der naturforschenden Gesellschaft in Danzig*, 1747, p. 507).

LEYDEN JAR

The discovery that water can be electrified, combined with the desire to preserve electric charges by imprisoning them within nonconductors, seems to have led to the discovery of the apparatus now known as the Leyden Jar, which originated independently and almost simultaneously in two different countries. It was first chanced upon in the latter part of 1745 by E. G. von Kleist, a clergyman of Pomerania. He tried the experiment of inserting, in a phial which he held in his hand, an iron nail, which he electrified from a machine. While still grasping the phial, he touched the nail with his other hand, when he received a severe shock the strength of which was increased upon repeating the experiment with some mercury or spirit of wine in the phial. The first printed account of Von Kleist's experiment is in J. G. Krüger's *Geschichte der Erde*, Halle, 1746 (*Anhang von der Electricität*, pp. 177–81, where a letter from Von Kleist to Krüger is quoted). What seemed most remarkable to Von Kleist was that the shock was obtained only when the bottle was held in the hand. If, after charging, it was placed upon a table, and a finger was held to the nail, no spark was perceived, but only a hissing noise; but if the bottle was then grasped in the hand once more, and the finger presented to the nail, an appreciable shock was felt. Von Kleist obtained the best results when he used the bulb and part of the stem of a disused thermometer for his experiment. The bulb was half filled with water into which was dipped a wire, the upper portion of which projected from the top of the stem, and was bent through a right angle, terminating in a small leaden ball. Von Kleist shortly afterwards communicated his discovery to some friends, through whom it came to the knowledge of Gralath of Danzig and of J. H. Winkler, who improved upon the original invention. In April 1746, Gralath discharged a number of Von Kleist's bottles by chains

formed by up to twenty persons holding hands, all of whom felt the shock simultaneously. The same result was obtained whether the people stood on the bare ground or on insulators, and whether they held one another's hands or the ends of long wires attached to their neighbours. But the experiment failed when the people stood round arm in arm, or when they were connected by non-conducting substances. It was noticed that persons forming links in such electric chains were not themselves electrified. Gralath formed batteries of Von Kleist's bottles, arranged in parallel, and discovered that "residual charges" remained in the bottles after their apparent discharge. He used such batteries to kill small birds, upon whose bodies his medical friends conducted *post mortem* examinations. In one of his experiments, Gralath persuaded a number of his friends each to take a Von Kleist's bottle and to hold the knob against the prime conductor of an electrical machine with one hand, while grasping a short piece of wire in the other. Another person then grasped all the free ends of these wires in one hand, and touched the prime conductor with the other hand, when he received a much more severe shock than his fellow-experimenters. In order to avoid painful shocks in such trials, Gralath decided to eliminate the human portion of the circuit. Winkler had wrapped a chain round the outsides of a number of Von Kleist's bottles, and had connected this to a conducting table, from which rose a metal rod forming a spark-gap with the prime conductor, to which the knobs of the bottles were applied. Upon operating the machine, sparks had appeared at the gap which could be heard a hundred paces away. This prompted Gralath to mount four Von Kleist's bottles in metal receptacles which were all connected by separate wires to a copper globe placed just under the prime conductor of a machine. To this conductor the knobs of the bottles were connected, and when the machine was set in motion, large sparks passed between the globe and the prime conductor. (See for Gralath, *Versuche und Abhandlungen der naturforschenden Gesellschaft in Danzig*, I, 1747, pp. 506–34; for Winkler, *Die Stärke der elektrischen Kraft des Wassers in gläsernen Gefässen*, Leipzig, 1746.)

In January 1746, Réaumur communicated to the Academy a letter which he had received from Musschenbroek of Leiden, of which the following is a translation of the essential portion: "I wish to report to you a new but terrible experiment, which I advise you on no account to attempt yourself. . . . I was carrying out some researches on the force of electricity; for that purpose I had suspended by two cords of blue silk, an iron gun-barrel, AB, which was receiving electricity by conduction from a glass globe which was being rapidly rotated on its axis, and rubbed meantime by the application of the hands. From the other end, B, there hung freely a brass wire, the
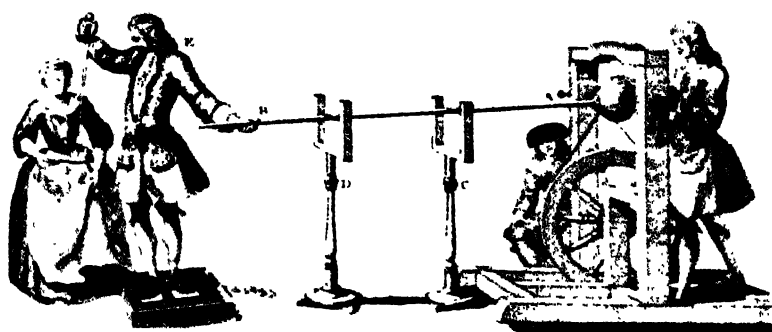
Gralath's Experiment
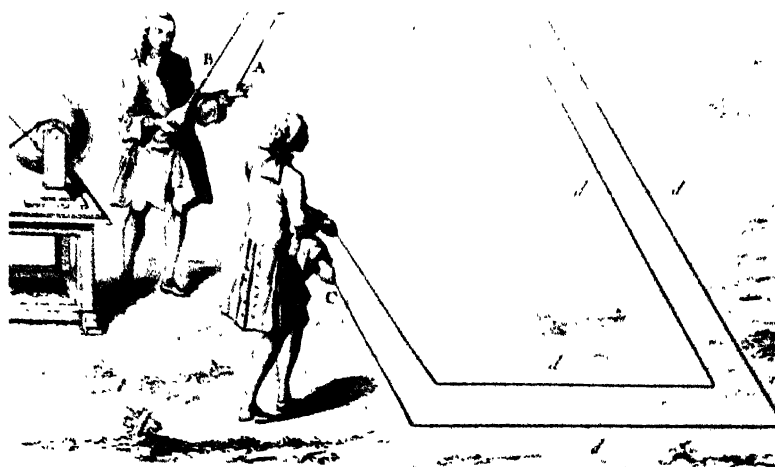
Le Monnier's Experiment
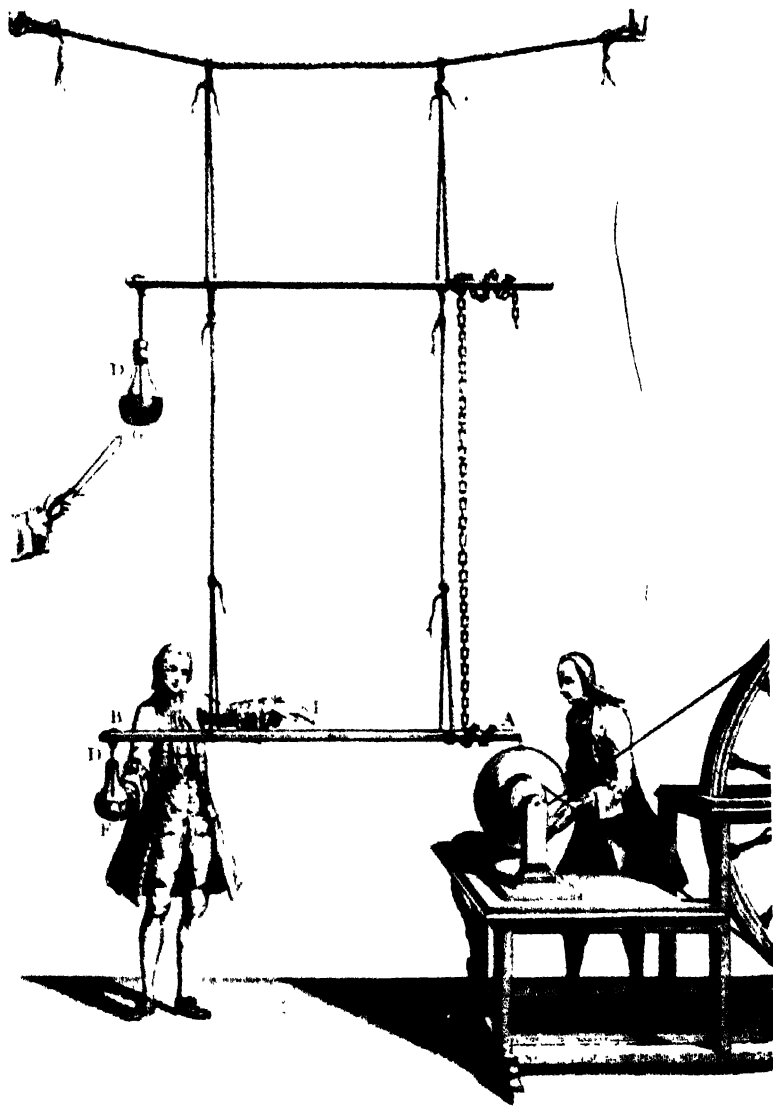
Musschenbroek's Experiment

end of which was immersed in a round glass vessel D, partly filled with water, which I was holding in my right hand F, while, with the other hand E, I tried to draw sparks from the electrified gun-barrel. Suddenly my right hand, F, was struck with such violence that my whole body was shaken as by a thunderbolt (Illustr. 105). The vessel, although made of thin glass, does not break as a rule, and the hand is not displaced by this disturbance, but the arm and the whole body are affected in a terrible manner which I cannot express; in a word, I thought it was all up with me." Musschenbroek went on to explain that, though the shape of the glass vessel did not seem to matter, the vessel must be of German or Bohemian glass, otherwise no effect was produced; not even Dutch glass would do. Some days after the reading of this letter, Nollet received an account from the physicist Allaman, who lived at Leiden, describing the same experiment. But in a subsequent letter to Nollet, Allaman stated that the first to discover the effect described in Musschenbroek's letter was one Cunaeus, a rich scientific amateur of Leiden. (See Abbé Nollet in *Mém. de l'Acad. Roy. des Sciences*, Paris, 1746, pp. 1–23.)

Undeterred by Musschenbroek's warning, Nollet and L. G. Le Monnier repeated the Leiden experiment forthwith. Nollet made it succeed, using ordinary French glass vessels, provided these were dry. He concluded that, in Musschenbroek's experiments, all except the German glass vessels must have been *moist*, which would account for their failure to give positive results. Nollet found that water was the best liquid with which to fill the vessel, but that other liquids (in particular, mercury) could be employed, provided that they were not sulphurous or oily, and that even powders or iron filings would serve. On the other hand, the vessel had to be made of glass or of porcelain; not even sulphur would serve as a substitute. The violence of the shock seemed to depend upon the size of the vessel. In the same volume of the *Mémoires* (pp. 447–64), Le Monnier describes his own early experiments with the Leyden jar. These mostly consisted of observations on the results of discharging jars through circuits composed of chains of people holding hands, or joined by chains or long wires which could be passed through wet grass or freshly dug earth, or wound round trees, without destroying the force of the shock. Le Monnier succeeded in transmitting the shock through water between wires dipping into the lakes at the Tuileries and the Jardin du Roi. In an attempt to ascertain the speed of propagation of the electric discharge, Le Monnier laid down two long parallel wires in a close at a Carthusian monastery. An observer grasped the two farther ends of the wire with his two hands, while the nearer ends were connected, one to the outside, and the other to the knob, of a charged Leyden jar (Illustr. 104). The observer was to judge

the interval between the instant at which the spark passed at the jar as the circuit was completed, and the instant at which he felt the shock; but all observers agreed that this interval was inappreciable.

These discoveries immediately aroused much interest throughout Europe, and led many amateurs to devote themselves to electrical experiments. In 1747, William Watson (1715–87) and several other Fellows of the Royal Society succeeded in sending an electric shock across the Thames. They discharged a jar through an external circuit which included a wire across Westminster Bridge, and which was completed through the bodies of three operators, two of whom dipped iron rods into the water on opposite sides of the river, 400 yards apart. When the circuit was closed all three felt a shock, and it was found that the discharge was sufficiently strong to fire spirit of wine. A similar experiment on a larger scale was carried out soon afterwards at Stoke Newington (*Phil. Trans.*, 1748, p. 49).
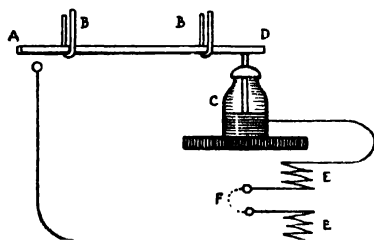
Illustr. 106.—Diagram to illustrate the arrangement of Watson's apparatus.

To Watson is due one of the early improvements in the form of the Leyden jar. Bevis had already coated the outside of the jar with foil; he even seems to have constructed condensers consisting of glass panes coated with metal foil. Watson gave the jar practically its present form by similarly lining the inside as well, and dispensing with the liquid contents of the jar (*Phil. Trans.*, 1748, pp. 92 ff.). He was also one of the pioneers in the early attempts to ascertain the velocity with which electricity is propagated in a wire. A negative result was obtained by Watson and other members of the Royal Society in experiments which they carried out in 1748 at Shooter's Hill over a circuit exceeding 2 miles in extent. Upon discharging the jar C (Illustr. 106) through the circuit CEFABD, the interval between the convulsive movement of an observer included in the circuit at F, and the passage of a spark at the distant gap A, was inappreciable, pointing to a velocity which was, at least, very great. These experiments were immediately repeated at various places in Europe, and, by Franklin, in America.

Franklin writes of his intention to make an exhibition of firing spirits by an electric spark sent through the water from side to side of the River Skuylkil, "an experiment which we some time since performed, to the amazement of many" (*Experiments and Observations on Electricity*, 5th ed., London, 1774, p. 37). But in reply to a proposal

Cavallo
(By courtesy of the Royal Society)

Benjamin Franklin

to measure the time taken by the discharge of a jar to travel round a circuit mostly consisting of North American rivers and brooks, and even including many hundreds of miles of ocean, Franklin wrote that such an experiment "only shows the extreme facility with which the electric fluid moves in metal; it can never determine the velocity." He explains this by means of an analogy: "If [a] tube be filled with water, and I inject an additional inch of water at one end, I force out an equal quantity at the other, in the very same instant. And the water forced out at one end of the tube is not the very same water that was forced in at the other end at the same time; it was only in motion at the same time" (*ibid.*, p. 290).

We may note in passing that Watson, whose activities in electrical research were many-sided, also observed about 1750 the luminous discharge of electricity through an exhausted glass tube nearly three feet long. The ends of the tube were closed by brass caps through which brass rods were inserted, whose distance apart could be varied, and one of which was connected to the prime conductor of an electrical machine. He concluded that it was the presence of the atmosphere which made it possible to accumulate a charge on a conductor (*Phil. Trans.*, 1751, p. 362). Lord Charles Cavendish observed a similar discharge through the Torricellian vacuum. The fact that electric sparks can be made to pass for a considerable distance through a partial vacuum had already been noticed by Grummert of Dresden (*Versuche u. Abhandl. d. naturf. Gesellschaft*, Danzig, 1747, p. 417). Watson's discovery eventually led to the invention of the Geissler Tube, and, in recent times, to the discovery of the cathode rays and X-rays. The *aurora borealis* is also now explained as due to electrical discharges through rarefied layers of the earth's atmosphere.

It was observed that Von Kleist's bottle (better known as Leyden jar) retained its charge for a longer period when the outside was insulated, but that in these circumstances it could not acquire a charge (L. G. Le Monnier, *Mém. de l'Acad. Roy. des Sc.*, 1746, pp. 447 f.). The first to give a clear explanation of these properties, and of the working of the Leyden jar in general, was Franklin. His theory of the nature of electricity, however, must be considered first.

THE NATURE OF ELECTRICITY

In the presence of such new and unprecedented discoveries eighteenth-century physicists soon began to enquire into the cause of electrical phenomena. The seventeenth-century thinkers had generally attributed them to quasi-material effluvia associated with charged bodies. Theories of this kind persisted to some extent even into the

eighteenth century. Thus the Abbé Nollet supposed that bodies might have two sets of pores, and that when they were charged effluvia might stream out from one set (seeming to repel neighbouring bodies) and in through the other set (carrying other bodies with them). His views appeared to be supported in some measure by the discovery that the rate at which a liquid flowed out of a containing vessel through a capillary tube was considerably increased when the whole was electrified. On the view that plant and animal bodies are systems of capillary tubes, Nollet concluded that electrification would probably accelerate the flow of sap in plants and the perspiration of animals. He sowed similar seeds in two similar pots which were kept under the same conditions, except that one was electrified for several hours every day for a fortnight and the other remained uncharged. He found that the electrified seeds sprouted two or three days earlier and grew more vigorously than the others. He next took pairs of various kinds of animals, weighed them, electrified one member of each pair for several hours, and then weighed them again. The electrified animal was usually found to have lost weight to an appreciable extent as compared with the other; and the same thing happened when the experiments were repeated with the conditions reversed for each pair (*Phil. Trans.*, 1748, p. 187).

On the supposition that electricity is a form of matter, it was to be expected that bodies should show an increase in weight upon being electrified; but all attempts to demonstrate this were unavailing. A similar result was obtained in the science of heat, when objects weighed in a heated condition, and again at ordinary temperature, showed no change in weight. It was, however, by no means inferred from these experiments that electricity and heat are mere conditions of bodies. The idea of imponderable substances, already assumed in attempts to explain the phenomena of light, were extended to account for electric, magnetic (related to electric), and thermal processes. The doctrine of imponderables dominated Physics until well into the nineteenth century. It was first shaken, as regards heat, by Rumford. Its complete overthrow in all branches is a problem which has occupied science down to the most recent times.

Although the doctrine of imponderables was not in a position to satisfy an advanced need of causality, yet in the stage to which knowledge had attained in the eighteenth century it offered the only possibility of an explanation. If light-phenomena were ascribed to the progressive motion of a special substance, it was necessary to postulate further substances as vehicles of heat and of electric and magnetic processes. The theory of electricity took a simpler form in the minds of those physicists who referred light-phenomena to a

wave-motion. Thus Euler was convinced that the source of all electrical processes was to be sought in the aether, in which, on his view as on that of Huygens, light is propagated. Euler held that electricity consists merely of a disturbance of the equilibrium of this aether. Bodies show one or the other electrical state according as aether is forced into them or driven out of them.

FRANKLIN

A similar conception guided the experimental enquiries of Benjamin Franklin, the first great American man of science. Franklin was born at Boston in 1706, the tenth son of a soap-boiler who had emigrated from England to escape religious persecution. He was early taken from school, and apprenticed to an elder brother who was a printer and publisher. After some youthful adventures and travels, in the course of which he worked for some time as a compositor in England, Franklin returned to Philadelphia, where he set up in business for himself and became the publisher of a newspaper. He quickly achieved success and began to play a prominent part in public life. His interest in electrical science was stimulated by the sight of some electrical apparatus which had been sent to the Philadelphia Library by one Peter Collinson, a London merchant and naturalist who became a Fellow of the Royal Society and with whom Franklin later corresponded. Franklin continued to occupy himself off and on for years with electrical experiments which he repeated in the presence of his friends. This experimental activity continued until about 1757, after which Franklin devoted himself almost entirely to the struggle for American independence from England, in securing which he played a leading part. In 1783 he signed the Peace Treaty in Paris, and subsequently returned to America to occupy a high position in the Government until his death in 1790, and to become for all time a great national hero.

Franklin believed in the existence of a single electric fluid pervading all bodies in varying quantities. He supposed that a body was electrically neutral when the fluid within it and without it was in equilibrium, but that if it contained more or less than the normal quantity the body would appear electrified one way or the other—*positively* for an excess, *negatively* for a defect, of the fluid. According to Franklin this fluid pervades the entire material universe, and is the cause of all electrical phenomena. "The electrical matter," he writes, "consists of particles extremely subtile. . . . Electrical matter differs from common matter in this, that the parts of the latter mutually attract, those of the former mutually repel each other. . . . But though the particles of electrical matter do repel each other,

they are strongly attracted by all other matter. . . . When a quantity of electrical matter is applied to a mass of common matter . . . it is immediately and equally diffused through the whole. . . . But in common matter there is (generally) as much of the electrical as it will contain within its substance. If more is added, it lies without upon the surface, and forms what we call an electrical atmosphere; and then the body is said to be electrified" (B. Franklin: *Experiments and Observations on Electricity made at Philadelphia in America*, 5th ed., London, 1774, pp. 54–5). When one body contains more of the electrical fluid in proportion to its size than another, then upon connecting the bodies by means of a conductor, or upon placing them sufficiently close together for a spark to pass, the fluid will flow from the former body to the latter until it is uniformly distributed between the two (*ibid.*, p. 39).

Franklin's theory, it is true, did not win universal assent. Several attempts were made to account for the existence of two different electrical states by postulating two distinct electrical fluids. Such a two-fluid theory was put forward in 1759 by Robert Symmer (*Phil. Trans.*, Vol. LI, p. 340). His attention had been drawn to the subject by the remarkably strong electrical effects which he observed upon drawing a black and a white silk stocking on to the same leg, removing them both, and separating them. By using several stockings he was able to charge a Leyden jar sufficiently to give a sharp shock or to ignite spirits of wine. In the theoretical part of his paper he states that "the operations of electricity do not depend upon one single positive power, according to the opinion generally received; but on two distinct, positive and active powers, which, by contrasting, and as it were counteracting each other, produce the various phenomena of electricity." A positively charged body, then, possesses a predominance of one sort of electricity, and a negatively charged one a predominance of the other sort, while in a neutral body the effects of the two fluids present just balance each other.

Pointless as the resulting dispute between the respective adherents of the one-fluid and of the two-fluid theories may have been, in the light of the evidence then available it had the effect of stimulating the experimental investigation of the phenomena bearing on the question. Thus Symmer himself sought experimental proof of his hypothesis by examining (with Franklin's assistance) the holes made by electric sparks in passing through quires of paper. He satisfied himself that the holes were due to the passage of something in *both* directions—from the negative to the positive side, as well as from the positive to the negative. Moreover those who postulated two fluids were at an advantage over those who postulated but one when it

came to explaining why two negatively charged bodies repel each other.

The experimental results obtained by Franklin in the years 1747-55 were described by him in numerous letters, mostly addressed to Collinson and by him communicated to the Royal Society, of which Franklin became a Fellow in 1756. The early letters deal with the charging of the Leyden jar, whose action he seeks to explain in terms of his one-fluid theory of electricity, while his later letters relate to his pioneer work in atmospheric electricity and to other matters of less interest.

Franklin's theory of the Leyden jar is best described in his own words:

"At the same time that the wire and top of the bottle, etc., is electrified *positively* or *plus*, the bottom of the bottle is electrified *negatively* or *minus*, in exact proportion; i.e., whatever quantity of electrical fire is thrown in at the top, an equal quantity goes out at the bottom. To understand this, suppose the common quantity of electricity in each part of the bottle, before the operation begins, is equal to 20; and at every stroke of the tube, suppose a quantity equal to 1 is thrown in; then after the first stroke, the quantity contained in the wire and upper part of the bottle will be 21, in the bottom, 19. After the second, the upper part will have 22, the lower 18, and so on, till, after 20 strokes the upper part will have a quantity of electrical fire equal to 40, the lower part none: and then the operation ends: for no more can be thrown into the upper part, when no more can be driven out of the lower part. If you attempt to throw more in, it is spued back through the wire, or flies out in loud cracks through the sides of the bottle.

"The equilibrium cannot be restored in the bottle by *inward* communication or contact of the parts; but it must be done by a communication formed *without* the bottle between the top and bottom, by some non-electric, touching or approaching both at the same time; in which case it is restored with a violence and quickness inexpressible. . . .

"As no more electrical fire can be thrown into the top of the bottle, when all is driven out of the bottom, so in a bottle not yet electrified, none can be thrown into the top, when none *can* get out at the bottom; which happens either when the bottom is too thick, or when the bottle is placed on an electric *per se*. Again, when the bottle is electrified, but little of the electrical fire can be *drawn out* from the top, by touching the wire, unless an equal quantity can at the same time get in at the bottom. . . .

"The shock to the nerves (or convulsion rather) is occasioned by the sudden passing of the fire through the body in its way from the

top to the bottom of the bottle. . . . But it does not appear from experiment that in order for a person to be shocked, a communication with the floor is necessary; for he that holds the bottle with one hand, and touches the wire with the other, will be shocked as much, though his shoes be dry, or even standing on wax, as otherwise. . . .

"Place an electrified phial on wax; a small cork-ball suspended by a dry silk thread held in your hand, and brought near to the wire, will first be attracted, and then repelled : when in this state of repellency, sink your hand, that the ball may be brought towards the bottom of the bottle; it will be there instantly and strongly attracted, till it has parted with its fire" (*op. cit.*, Letter III).

In his further experiments Franklin also employed as a condenser a pane of glass coated with lead on both sides, which is still known as a "Franklin's Pane," though anticipated by Smeaton and Bevis.

Franklin's popular fame, at least, rests chiefly upon the experiments by which he succeeded in proving that lightning is an electric discharge.

When the Greek philosophers sought to substitute a causal explanation of the processes of nature for mythical accounts of them, they attributed thunderstorms to sulphurous inflammable vapours which accumulated in the clouds and broke through them as lightning. Even in the seventeenth century this notion still persisted, and the true nature of the phenomenon remained unsuspected. According to Descartes, a thunderstorm was due to the upper clouds falling down on to the lower ones. Euler relates that the first men to suspect a connection between the electric spark and lightning were regarded as dreamers. But what even at the beginning of the eighteenth century was advanced as a mere conjecture, was placed by Franklin's experiments upon a basis of certainty.

Already in 1708 Wall had described in the *Philosophical Transactions* (Vol. XXVI, p. 69) how he had produced flashes of light, with a crackling sound, upon drawing a long piece of amber through a piece of woollen material, and how sparks an inch long could be drawn from the excited amber by holding the finger a little distance away. He likened this effect to thunder and lightning. Newton described a similar experiment and drew the same comparison in 1716. Another precursor of Franklin in this field was the German physicist Winkler, who in 1746 discussed the question "whether the shock and spark of the concentrated electricity (in Von Kleist's bottle) are to be regarded as a sort of thunder and lightning?" (*Die Stärke d. elektr. Kraft d. Wassers etc.*, Leipzig, 1746; also in Hellmann's *Neudrucke*, No. 11). Winkler came to the conclusion that a

thunderstorm and an artificially induced electric discharge differ
from each other in their intensity only, not in their nature. He
considered the evaporation of water and the resulting friction to be
the source of the electricity associated with thunderstorms.

Franklin first declared in favour of the electrical nature of thunder-
storms in 1749, when he set forth the following evidence for the
correspondence between lightning and the electric spark:

(1) The resulting light and sound are similar, and both pheno-
mena are practically instantaneous.

(2) The spark, like lightning, is able to set bodies on fire.

(3) Both can kill living creatures. (Franklin killed a hen by
the discharge of several Leyden jars.)

(4) Both do mechanical damage and give a smell like burnt
sulphur (the investigation of which later led to the discovery
of ozone).

(5) Lightning and electricity follow the same conductors and
pass most readily to sharp points.

(6) Both are able to destroy magnetism, or even to reverse the
polarity of a magnet.

(7) Both are able to melt metals. (*Experiments and Observations*,
5th ed., p. 331.)

Franklin's experimental proof of this last point was connected
with a controversy which he had with his friend and fellow-experi-
menter Kinnersley. Franklin's method of melting metals by the
electric spark was to place a leaf of tin or gold between two glass
discs and then to discharge a large Leyden jar through the leaf.
The metal was pulverized; and as Franklin could not detect any
heat arising from the process, he called it a "cold fusion." He sup-
posed that the electric fluid penetrated the pores between the
particles of the metal, whose cohesion was thereby destroyed.
Kinnersley showed, however, by discharging a battery of thirty-five
jars through a wire, that metals could be brought to red heat and
even melted in the process. This experiment convinced Franklin that
electricity does in fact melt metals by heating them, and he gave
up the notion of "cold fusion."

Franklin gave a direct proof of the association of electricity with
thunder-clouds by the famous kite experiment, which he carried
out in June 1752. His procedure may be understood from the
instructions for repeating the experiment which he sent to Collinson
later in the year (Oct. 19, 1752) and which were published in the
Philosophical Transactions (Vol. XLVII, p. 565).

"Make a small cross," he writes, "of two light strips of cedar,

the arms so long as to reach to the four corners of a large thin silk handkerchief when extended; tie the corners of the handkerchief to the extremities of the cross, so you have the body of a kite; which being properly accommodated with a tail, loop, and string, will rise in the air, like those made of paper; but this being of silk is fitter to bear the wet and wind of a thunder-gust without tearing. To the top of the upright stick of the cross is to be fixed a very sharp pointed wire, rising a foot or more above the wood. To the end of the twine, next the hand, is to be tied a silk ribbon, and where the silk and twine join, a key may be fastened. This kite is to be raised when a thunder-gust appears to be coming on, and the person who holds the string must stand within a door or window, or under some cover, so that the silk ribbon may not be wet; and care must be taken that the twine does not touch the frame of the door or window. As soon as any of the thunder clouds come over the kite, the pointed wire will draw the electric fire from them, and the kite, with all the twine, will be electrified. . . . At [the] key, the phial may be charged; and from electric fire thus obtained spirits may be kindled, and all the other electric experiments be performed, which are usually done by the help of a rubbed glass globe or tube, and thereby the sameness of the electric matter with that of lightning completely demonstrated" (*op. cit.*, Letter XI).

Franklin later discovered that thunder-clouds are sometimes positively and sometimes negatively charged. This was confirmed by Canton in England (*Phil. Trans.*, 1753, p. 350). At first Franklin supposed that the electricity of thunder-clouds was carried up by vapours rising from the ocean, where it was produced from the friction of the salt and the water, and where it further showed itself as marine phosphorescence. Later, however, he gave up this idea upon finding that sea-water lost its luminosity when kept in a bottle for some hours. A Frenchman, De Romas, repeated Franklin's kite experiment on a large scale in the summer of 1753. He sent up a kite 7½ feet long to an altitude of 550 feet on a 780-foot cord twisted round an iron wire. The cord was fastened to a metal tube from which sparks 8 inches long were drawn. (See Nollet: *Lettres sur l'Electricité*, II, p. 239.) It is noteworthy that already some years before performing his kite experiment, Franklin had suggested that electricity might be collected from thunder-clouds by means of pointed iron rods suitably mounted: "To determine the question, whether the clouds that contain lightning are electrified or not, I would propose an experiment to be tried where it may be done conveniently. On the top of some high tower or steeple, place a kind of sentry-box big enough to contain a man and an electrical stand. From the middle of the stand let an iron rod rise and pass bending

out of the door, and then upright 20 or 30 feet, pointed very sharp at the end. If the electrical stand be kept clean and dry, a man standing on it when such clouds are passing low, might be electrified and afford sparks, the rod drawing fire to him from a cloud" (*Experiments and Observations*, 5th ed., p. 66).

Atmospheric electricity seems first to have been collected by means of such a rod (prior to Franklin's kite experiment) by the French botanist T. F. Dalibard. At the request of Buffon, Dalibard translated into French some of the early letters of Franklin which Collinson had brought out in 1751, and he was led to perform electrical experiments on his own account. He described these in the second edition of his translation (Paris, 1756), and the portion relating to his experiments on atmospheric electricity is reprinted in Hellmann's *Neudrucke*, No. 11. Dalibard relates how he set up at Marly, near Paris, a pointed iron rod about forty feet high and about an inch in diameter, bound by silken cords to dry piles, and having its lower extremity bent in so as to rest on an insulated table in a little wooden shelter. He hoped to see luminosity at the point, and to draw sparks from the foot, of the rod. He committed the observations in his absence to a local ex-dragoon named Coiffier, who was to draw off the sparks in safety by means of a brass wire attached to a bottle as insulator. Coiffier made the expected observation on May 10, 1752, during a thunderstorm. The clergyman attended as a witness, and himself drew sparks about 1½ inches long from the lower end of the rod; he estimated the interval between successive sparks as equivalent in duration to a *pater* and an *ave*. He afterwards found a weal on his arm where he had received a spark, and his friends told him that he smelt of sulphur.

Similar observations were made a week later in Paris by Delor, using a 99-foot bar of iron standing upright on a cake of resin. Dalibard's success also stimulated L. G. Le Monnier to investigations on atmospheric electricity, which he described in *Mém. de l'Acad.* 1752, pp. 233 f.; reprinted by Hellmann, *op. cit.*). In an open space at St. Germain he set up a 32-foot pole with a metal point from which a thin wire about fifty fathoms long descended, and was attached to a horizontal silken cord. He obtained sparks from this wire during a storm on June 7, 1752; they had all the characteristics of ordinary electricity. Le Monnier found that quite short pointed iron rods (only four or five feet above the level of the ground) collected electricity in time of storm; and eventually he found that he himself, standing on an insulator in the middle of the garden and holding up one hand, became electrified. In July he found that dust was attracted to the wire of his apparatus even in perfectly calm weather, though he did not at first realize the full significance of this

as indicating that the atmosphere is more or less electrified at all times.

Most of the early attempts made in England to collect atmospheric electricity, by Watson and other Fellows of the Royal Society, were frustrated by a scarcity of thunderstorms and by the wetting of the apparatus by rain. The first English investigator to succeed seems to have been John Canton. He wrote to Watson (July 21, 1752) as follows: "I had yesterday, about five in the afternoon, an opportunity of trying Mr. Franklin's experiment of extracting the electrical fire from the clouds; and succeeded, by means of a tin tube, between three and four feet in length, fixed to the top of a glass one, of about eighteen inches. To the upper end of the tin tube, which was not so high as a stack of chimneys on the same house, I fastened three needles with some wire; and to the lower end was soldered a tin cover to keep the rain from the glass tube, which was set upright in a block of wood. I attended this apparatus as soon after the thunder began as possible, but did not find it in the least electrified, till between the third and fourth clap; when applying my knuckle to the edge of the cover, I felt and heard an electrical spark; and approaching it a second time, I received the spark at the distance of about half an inch, and saw it distinctly. This I repeated four or five times in the space of a minute, but the sparks grew weaker and weaker; and in less than two minutes the tin tube did not appear to be electrified at all. The rain continued during the thunder, but was considerably abated at the time of making the experiment" (*Phil. Trans.*, 1752, p. 567).

Such experiments, however, were more dangerous than was suspected. In the following year, 1753, Richmann of St. Petersburg was examining, during a thunderstorm, an electrometer attached to the lower end of a conductor which he had set up on his roof, when he received from the conductor a shock which instantly killed him.

Franklin's experiments soon suggested to him the idea of using lightning conductors for the protection of buildings, ships, etc. The earliest reference to lightning conductors is found in Franklin's *Opinions and Conjectures*, sent to Collinson under date July 29, 1750 (see *Experiments and Observations*, 5th ed., p. 65). After describing how a charged conductor may be discharged by presenting to it the point of a needle some distance away, Franklin points out that if lightning is the same thing as electrical fire, the conductor in the experiment might represent electrified clouds, and "may not the knowledge of this power of points be of use to mankind, in preserving houses, churches, ships, etc., from the stroke of lightning, by directing us to fix on the highest parts of those edifices, upright rods of iron made sharp as a needle, and gilt to prevent rusting,

and from the foot of those rods a wire down the outside of the building into the ground, or down round one of the shrouds of a ship, and down her side till it reaches the water? Would not these pointed rods probably draw the electrical fire silently out of a cloud before it came nigh enough to strike, and thereby secure us from that most sudden and terrible mischief?" The project attracted widespread attention in America and soon afterwards in Europe. Franklin's plan was to fasten one or more iron rods to the exterior of a building in such a manner as to provide the lightning with an unbroken conducting path from the top of the building to the wet subsoil. There the rod was to be bent outwards so as to save the foundations from injury.

Franklin was led to set up lightning conductors largely as a result of his investigations on the electric discharge which occurs from a sharp point on a charged conductor, and which may give rise to an electric wind—an effect frequently employed in the latter part of the eighteenth century to operate a sort of electric Catharine-wheel. He tried to explain the phenomenon on the supposition that near the point there is not sufficient matter to overcome by its attraction the mutual repulsion of the electric particles, which are therefore shed abroad into the surrounding air.

A sharp controversy arose in England about 1780 as to whether the tops of protective lightning conductors should be made pointed or blunt. The controversy arose chiefly out of the problem of securing the powder-magazines at Purfleet from destruction by lightning. Franklin advocated pointed conductors, and these were finally adopted; but they were opposed by Benjamin Wilson and others as tending to attract lightning that might otherwise have passed over harmlessly. Both sides staged experiments in which models of the magazines, fitted with conductors of several patterns, were exposed to "thunder-clouds" consisting of charged and insulated vessels of water which slid on frames overhead. These experiments, however, led to no very convincing results. (See the *Philosophical Transactions* of the period.)

B. Induction and Pyro-Electricity

Among the many contemporaries of Franklin who occupied themselves with electrical experiments, two continental physicists, Wilcke and Aepinus, stand out prominently.

WILCKE

Johan Carl Wilcke (1732–96) was of German origin, but spent most of his life in Sweden, where he became Secretary of the

Academy of Sciences. In this capacity he gave lectures on physics at Stockholm. In his *Dissertatio inauguralis de electricitatibus contrariis* (Rostock, 1757), Wilcke established the important result that upon rubbing two bodies together both kinds of electrification are invariably produced. He arranged the bodies which he investigated in a series of which each member, if rubbed with a body lower in the series, became positively electrified, while if rubbed with a body higher in the series, it became negatively electrified. The following members of the series, for example, were arranged in the order: glass, wool, wood, sealing-wax, metal, sulphur. Wilcke's was the first of a long succession of attempts to construct such a series, of which those of Young and of Faraday are the best known. The place occupied by any given substance in such a list, however, is not quite invariable, since it depends, not only upon the nature of the substance, but also upon the condition of the surfaces between which the friction takes place. John Canton (1718–72), an ingenious English experimentalist who worked on similar lines to Wilcke and Aepinus, also drew attention to the fact that the charge generated by rubbing a given substance may be either positive or negative according to the nature of the surface and the rubber employed. He demonstrated this by generating charges of opposite sign on the two opposite ends of the same glass tube (*Phil. Trans.*, 1754, p. 780). Wilcke inclined towards the two-fluid theory of electricity. He adduced in its favour the fact that a luminous discharge and an accompanying air-current are observed to flow out from a sharp point on a charged and insulated conductor even when the latter is negatively charged, which was not then readily explicable on Franklin's theory.

Wilcke discovered a new way of generating electricity. He found that both sulphur and resin, when melted and allowed to solidify in an insulating porcelain vessel, became strongly electrified negatively—a property characteristic of fusible non-conductors. (See *Konigl. Svenska Vetenskaps Academiens Handlingar*, or the German transl. by Kästner.)

AEPINUS

Wilcke's colleague in many of his researches, Franz Ulrich Theodor Aepinus (1724–1802), was Professor of Astronomy at the Berlin Academy, and later settled at St. Petersburg, where he taught physics and superintended the Normal School. He wrote numerous papers on electricity and astronomy, but his most important book was his *Tentamen theoriae electricitatis et magnetismi* (St. Petersburg, 1759). His theory bears a considerable resemblance to that of Franklin, as it postulates an all-pervading electric fluid

which is made up of particles repelling one another and attracting those of ordinary matter, and which tends to distribute itself in a state of equilibrium. Aepinus' principal contributions to electrical knowledge, however, concern what are now called electrical induction and pyro-electricity.

About 1753 Canton had studied the effect of bringing a charged body up to a pair of cork balls, suspended by linen threads, in contact with each other. He found that in these circumstances the balls repelled each other, although no electricity was communicated to them from the charged body; but that, upon the withdrawal of the latter, they came together again (*Phil. Trans.*, 1753, p. 350). He attributed the phenomenon to the agency of the "electric atmosphere" surrounding the charged body—a usual conception at that time. Canton also observed that upon bringing a charged body up to an insulated neutral conductor, the latter developed two opposite charges, the one nearest the influencing charge being of opposite sign to it, and the one farthest away being of the same sign. When the influencing charge was removed the conductor became neutral again.

Canton's experiments were repeated more precisely by Wilcke and Aepinus. Wilcke observed that when a neutral body, brought into the neighbourhood of a charged one, is momentarily earthed, it acquires a charge opposite to that of the influencing body. Aepinus explained this as due to the expulsion of fluid from the neutral body by the superabundant fluid in the charged one. Wilcke and Aepinus constructed an early form of parallel-plate condenser, and at the same time disproved the prevailing view that the glass composing a Leyden jar was indispensable to the accumulation of opposite charges on the outer and inner linings. They coated two boards with metal and hung them parallel to each other a few inches apart. They insulated and charged one and earthed the other. Upon touching both simultaneously the operator received a severe shock. Aepinus inferred that all that was necessary to permit of the accumulation of electricity in this way was a pair of conductors separated by a non-conductor. With Wilcke he performed some interesting experiments on a Franklin's Pane from which the metal coatings could be removed at will.

Aepinus had very sound opinions on the relation between conductors and non-conductors. He recognized that it was impossible to draw any hard-and-fast distinction between them. The difference depends merely upon the relative resistance offered to the passage of a charge through different substances. Conductors offer very little resistance; non-conductors, on the other hand, offer very appreciable resistance, and hence a discharge through these occupies

much more time. These ideas were later made the basis of Faraday's theory of residual charges.

The discovery of electrification by induction suggested a certain (though somewhat illusory) analogy between the actions of electric charges and of magnetic poles. A rather similar analogy was presented by the phenomena of pyro-electricity, which were first carefully studied during the eighteenth century.

It had long been known to jewellers, who were in the habit of testing gems by fire, that tourmaline, when laid upon glowing coals, attracts ashes and forthwith repels them. This peculiar effect, similar to the action of a charged body upon a suspended pith-ball, and suspected, even at the beginning of the eighteenth century, to be electric in nature, was carefully investigated by Acpinus (again in collaboration with Wilcke), who published his results in 1756 (*Hist. de l'Acad. de Berlin*, p. 105) and 1762 (*Recueil de différents mémoires sur la Tourmaline, St. Pétersbourg*). It was found that upon warming the crystal, one end became positively, and the other end negatively, electrified. These electrifications were likened to the opposite polarities of the two ends of a magnet. This phenomenon was later studied by Bergman of Uppsala, who showed that it depends, not upon the absolute temperature of the crystal, but upon changes of temperature (*Swedish Acad.*, 1766). So long as the temperature remains constant, at whatever level, the crystal remains neutral. During an increase of temperature, one end is positive and the other negative; during a decrease, these signs are reversed. Similar results had been obtained by Wilson (*Phil. Trans.*, 1759, p. 308, and 1762, p. 443), and also by Canton, who showed that the charges appearing on a pyro-electric crystal are not only opposite but equal (*Phil. Trans.*, 1762, p. 457). Other such gems were found in course of time; and Haüy, about 1800, sought to relate their pyro-electric properties to their crystalline form. The phenomena of pyro-electricity, however, have turned out to be highly complicated.

PHYSICS:

IV. ELECTRICITY AND MAGNETISM (II)

C. ELECTROSTATICS

THE remarkable development of electrical science which had continued throughout the course of the eighteenth century culminated towards its close in the determination of the precise law of force between electric charges. Electrical phenomena, which had hitherto been described qualitatively, were henceforward to be subject to exact mathematical investigation. and a new science of *Electrostatics* arose. With this culminating phase are especially associated the names of Priestley, Cavendish, and Coulomb. Though strongly contrasted in character and in outward circumstances, the Yorkshire schoolmaster, the wealthy London recluse, and the French military engineer, each contributed to the establishment of the law of electric force. And a further link between Priestley and Cavendish is to be found in the fact of their having both investigated the laws of resistance of conductors to electrostatic discharges.

PRIESTLEY

Joseph Priestley, as a young schoolmaster, experimented with an electrical machine; and later, in the course of repeated visits to London, he made the acquaintance of Franklin, Watson, Canton, and other leading electricians of the time. It was with their encouragement that he produced, after less than a year's work, his first scientific book, *The History and Present State of Electricity, with Original Experiments* (London, 1st ed., 1767; 3rd ed., 1775). The first of the two volumes into which this successful work expanded presents a lucid survey of the development of electrical science down to Priestley's own day. This portion was compiled as far as possible from original sources, many of which were put at Priestley's disposal by his scientific friends. In his second volume, Priestley sets out some general propositions tending to systematize the mass of experimental facts already brought to light; he reviews the current theories of the nature of electricity; he throws out queries suggesting further lines of enquiry; he gives a valuable illustrated account of contemporary electrical machines and other apparatus, with advice to amateur investigators; and he describes the numerous electrical experiments which he himself was led to perform in the course of his enquiries, and which had won him election to the Royal Society

in 1766. Though inclining towards the one-fluid theory of electricity, Priestley adopted a critical attitude towards all such hypotheses. "By *electricity*," he writes, "I would be understood to mean, only those *effects* which will be called electrical; or else the *unknown cause* of those effects" (*History*, 3rd ed., II, p. 3). He thought the function of hypotheses in science was to suggest lines of enquiry leading to the establishment of new facts, whereby the hypothetical element would be progressively eliminated.

Among the more noteworthy of Priestley's electrical observations, described in his book and in memoirs contributed to the *Philosophical Transactions*, the following may be mentioned. Following Franklin, he observed the currents of air emanating from projecting points upon conductors which had received either positive or negative charges, and he showed that these currents were sometimes sufficiently strong to blow out a lighted candle, or to turn a small vaned wheel. He drew attention to the rings of prismatic colours (arranged as in a rainbow) which are formed on metal surfaces round points at which powerful sparks have repeatedly passed between the metal and a neighbouring conductor (*Phil. Trans.*, 1768, p. 68). He points out their resemblance to Newton's Rings; and in an account given in the *History* he even suggests that perhaps the "fairy rings" in pastures may be an effect of this sort caused by lightning (to whose agency such rings were at that time frequently attributed). Priestley's experiments on electrical conduction are of greater significance. He showed that carbon is a good conductor of electricity (*op. cit.*, Vol. II, p. 193), and that dry ice and red-hot glass must also be classed among conductors. In memoirs contributed to the *Philosophical Transactions* (1769, pp. 57 and 63), Priestley begins by describing his investigations of the "lateral force of electrical explosions," whereby light objects are scattered by an electric discharge in their neighbourhood, though not themselves acquiring any charge. He attributes this effect to the expulsion of air from the place where the "explosion" occurs. His attempt to show that such scattering did not occur in a vacuum, however, was inconclusive owing to the imperfection of his pump. He next experimented to see whether the force of the discharge of a battery of jars, measured by its capacity to melt wire included in the external circuit, was diminished when the circuit was given a zig-zag configuration. He found what length of small iron wire he could just melt by discharging a certain battery through it, the rest of the circuit being composed of 3 yards of thick brass wire. He then bent this brass wire into sinuosities, but he found the length of iron wire just melted by the discharge to be the same as when the brass wire was straight. He observed, however, that the force of the discharge

measured in this way, did depend upon the *length* of the external circuit; and he sought to measure the obstruction to the discharge presented by the mere length of a conductor, by finding across what width of air-gap the discharge would pass in preference to traversing a long metallic circuit. Taking a long wire, he arranged it in the form of a loop, so that one of its ends could be connected with the outside of the battery of jars, and the other with the inside. He repeatedly discharged the battery, beginning with the ends of the wire in contact with each other, and progressively separating them between successive discharges until a spark no longer passed and the whole of the discharge went through the wire. The maximum width of the gap across which the spark would just pass appeared proportional to the length and thinness of the wire forming the circuit. Priestley studied in some detail a pheno-menon which he termed the *lateral explosion*, and which is now some-times called the *side-flash* (*Phil. Trans.*, 1770, p. 192). He had learned from his own experiments and those of Wilson that, when Leyden jars are discharged through imperfectly conducting circuits, the operator often feels a slight shock, though his body does not form part of the circuit. To ascertain whether electricity actually passed from the circuit to neighbouring bodies, Priestley proceeded by insulating a neutral conductor in the neighbourhood of a circuit through which a discharge was to pass, and observing whether a pith-ball electroscope connected to it indicated any gain or loss of charge when the discharge of the battery took place. His con-ductor was a tube of pasteboard 7 feet long and 4 inches in dia-meter, covered with metal foil and suspended on silken threads within $\frac{1}{4}$ inch of a metal rod connected to the outside of the battery, which was then discharged through an interrupted circuit. A spark was observed to pass from the rod to the insulated tube, but no divergence of the attached pith-balls was observed, although they were well able to show the presence on the conductor of much less electricity than was represented by the spark. This experiment was repeated over fifty times, being varied in several respects, but without giving a significantly different result. Priestley repeated the experiment *in vacuo*, passing a spark between two rods separated by a gap; and when he had reduced the width of the gap to 2 inches, he observed that it was bridged by a uniform "thin blue or purple light." There was no distinction between the appearances at the respective terminals such as occurs in an ordinary discharge at low pressure. "In all other cases," Priestley writes, "the electric matter rushes in a single direction; whereas in this it goes and returns in the same path; and, as far as can be distinguished, in the same instant of time, so that all the differences of the two electricities, which are

so conspicuous *in vacuo*, must here be confounded." This pheno-
menon, apparently the first oscillatory discharge to be recognized
as such, was independently investigated by several physicists during
the succeeding century.

Perhaps the most interesting of all Priestley's contributions to
electrical science, however, was his attempt to prove experimentally
that electrical attractions and repulsions vary with distance according
to the inverse square law. Franklin had reported to Priestley that
he had been unable to detect any electrification in cork balls
enclosed in a charged metal vessel; and Priestley followed up this
observation, as he relates at the close of his *History*. "Accordingly,
December 21st [1766] I electrified a tin quart vessel, standing
upon a stool of baked wood; and observed, that a pair of pith-balls,
insulated by being fastened to the end of a stick of glass, and hanging
entirely within the cup, so that no part of the threads were above the
mouth of it, remained just where they were placed, without being
in the least affected by the electricity"; though Priestley found that
the balls did acquire a slight charge if earthed, or withdrawn from
the cup, or touched against its side, before the cup was discharged.
This, however, could be attributed to the cup not being entirely
closed; and Priestley asks: "May we not infer from this experiment,
that the attraction of electricity is subject to the same laws with that
of gravitation, and is therefore according to the [inverse] squares of
the distances; since it is easily demonstrated that, were the earth
in the form of a shell, a body in the inside of it would not be attracted
to one side more than another" (*History*, II, pp. 372 f.). Priestley's
attempt to establish the inverse square law for electrical forces may
be regarded as the germ of Cavendish's more refined investigations
on this matter.

CAVENDISH

Pioneer work of the greatest importance was done in electrical
science during the seventeen-seventies by Henry Cavendish (1731–
1810), the son of Lord Charles Cavendish, himself an experimentalist
of distinction. Henry's electrical experiments were performed in his
father's London house; but unfortunately he seems to have carried
them out merely to satisfy his own curiosity, and he did not publish
the most significant of his results, though he had ample opportunity
for doing so. Hence when at length they saw the light in Maxwell's
edition of 1879, they had been for the most part rediscovered by the
independent researches of Faraday (see *The Electrical Researches of
the Hon. Henry Cavendish*, edited by J. Clerk Maxwell, Cambridge,
1879). Cavendish himself published only two papers on electricity,
and these were contributed to the *Philosophical Transactions*.

In the first and more important of these papers, published in 1771 and entitled *An Attempt to Explain some of the Principal Phenomena of Electricity, by means of an Elastic Fluid* (*Phil. Trans.*, Vol. LXI, p. 584), he sought to lay the foundations of a mathematical theory of electrostatics. His underlying hypothesis, similar to those of Franklin and Aepinus, was that electricity is a fluid whose particles repel one another, and attract the particles of ordinary matter, with a force varying inversely as some power of the distance less than the cube, while the particles of matter repel one another in accordance with the same law. In an electrically neutral body, the quantity of electric fluid is such that the attraction which it exerts upon any given particle of matter just balances the repulsion which the remaining matter exerts upon that particle. In electrified bodies the amount of fluid exceeds or falls short of this amount. He investigates mathematically the distribution of fluid in charged conductors, the forces exerted by various distributions of matter or fluid upon particles or upon each other, the movements of fluid between communicating charged conductors, and so forth. He confirms his theoretical conclusions, as far as possible, from the results of familiar experiments.

In his second paper, published in 1776 (*Phil. Trans.*, Vol. LXVI, p. 196), Cavendish describes how he made an artificial "torpedo" of leather, connected by insulated wires with a battery of Leyden jars, and how he succeeded in reproducing the functions of the fish even when the apparatus was immersed in salt water.

Cavendish's unpublished experiments deal chiefly with the comparison of what we should call the *capacities* of conductors and condensers of divers shapes and sizes, and also with the comparison of the resistance offered by columns of conducting solutions to the passage of electrostatic charges, and with the laws of electric force.

Cavendish's unpublished work in electrostatics was based on what he called the "degree of electrification," which he likened to the pressure in a fluid, and which is comparable to the modern conception of *tension* or *potential*. If two charged conductors are placed in communication, electricity will flow between them until this pressure is everywhere the same, when their "degrees of electrification" will be equal. The charges on the two conductors, however, will not in general be equal, but will depend on the shape and size of the conductors. Much of Cavendish's work in electrostatics consisted in finding the ratio of the charge on each of a number of conductors to that on a standard conductor (a sphere 12·1 inches in diameter covered with tinfoil) placed in electrical communication with it. When this ratio was determined it was easy to calculate the diameter of the sphere which, when electrified to an equal

degree, would have the same charge with the given conductor, since the charges on equally electrified spheres are proportionate to their diameters. Cavendish was thus able to anticipate the modern measurements of *capacity*, and to measure it in "*inches of electricity.*" He found that the capacities of his condensers (which were discs of tinfoil separated by non-conducting material) depended to some extent on the substance employed to separate the coatings; and he found the numerical ratio of the charges obtained, using given separating substances, to the charges obtained when air was employed, under otherwise similar circumstances. He thus anticipated to some extent Faraday's work on specific inductive capacity. In using condensers of the type described, he found it necessary to allow for the spreading of the charges over the glass. He devised a special apparatus and method for measuring capacities; but his accuracy, though remarkable, must have been impaired by his use of crude electrometers consisting merely of pairs of suspended cork or pith-balls.

In order to compare the conducting powers of various salt and acid solutions of known strengths, Cavendish placed them in glass tubes about a yard long which were corked at each end. Through the corks wires were passed which could be pushed in, or partly withdrawn, so as to vary the effective length of the liquid column. The wires from one end of each of two tubes of solution to be compared were connected to the earthed outsides of a set of Leyden jars, all charged up to the same tension. Cavendish, taking a piece of metal in each hand, then touched with one piece the free wire of one of the tubes, and with the other the knob of one of the jars, when he obtained a shock. He repeated the experiment, making the circuit through the second tube, then again through the first tube, and so on alternately, discharging one of the jars each time until all had been discharged. He judged which tube gave the greater shock, and concluded that that tube offered the smaller resistance. He next varied the length of solution through which the shock had to pass in one of the tubes so as to make the shocks more nearly equal, and went through the process again and again until approximate equality had been reached, when the lengths of the columns could be compared. In like manner Cavendish studied the variation of resistance with temperature. He partially anticipated Ohm's Law by showing that the resistance of a conductor is independent of the strength of the discharge; and he gave the laws according to which the discharge divides itself among a number of conductors in parallel. This work and that described in the next paragraph remained unpublished.

Cavendish proved that the charge upon a conductor resides entirely upon its surface, and he thence deduced the inverse square

...*r* of electrical repulsion. He took his 12·1 inch globe covered with tinfoil, which served also as his standard of capacity, and having insulated it, he enclosed it within two hinged pasteboard hemispheres which, however, did not touch it at any point. He then electrified the hemispheres, connected them momentarily by a wire with the inner globe, and then, having separated the hemispheres,
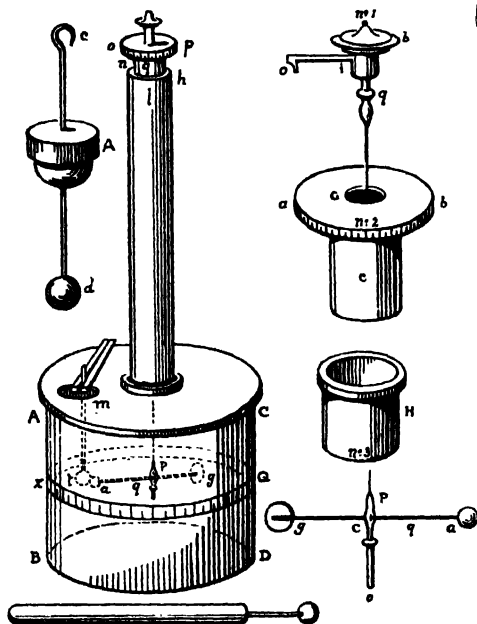

Illustr. 111.—Cavendish's Experiment on the Law of Electric Force

tested the globe with a pith-ball electroscope. He found it to be uncharged, and showed that from this result, if exactly true, the inverse square law of repulsion must follow. From some quantitative experiments with this apparatus he was able to conclude that the electric force must at least vary inversely as some power of the distance lying between $2 + \dfrac{1}{50}$ and $2 - \dfrac{1}{50}$. Maxwell, by a similar experiment, later reduced the limits of uncertainty to $\pm \dfrac{1}{21,600}$.

COULOMB

Charles Augustin Coulomb (1736–1806), a native of Angoulême, began his career as a military engineer, and his scientific interests and enquiries, like those of Von Guericke, grew out of his work. Upon returning in 1776 from a spell of duty at Martinique, he settled in Paris, where he devoted himself to researches dealing chiefly with friction, torsion, and the rigidity of materials, which won him a prize and the membership of the Academy of Sciences. Coulomb was attracted to the problems of electricity and magnetism through the offer of a prize by the Academy for the best method of constructing a ship's compass. It was while investigating this problem, and applying thereto the results which he had obtained on rigidity (especially torsional rigidity), that Coulomb, about 1784, invented his torsion-balance. A torsion-balance was also invented

by John Michell, probably before Coulomb, but there seems to be no evidence that the two men did not arrive at the invention of the instrument independently. Cavendish writes in his paper on the mean density of the Earth, "Many years ago, the late Rev. John Michell of this [the Royal] Society contrived a method of determining the density of the Earth, by rendering sensible the attraction of small quantities of matter; but, as he was engaged in other pursuits, he did not complete the apparatus till a short time before

Illustr. 112.—Coulomb's Torsion-Balance

his death, and did not live to make any experiments with it. After his death the apparatus came to the Rev. Francis John Hyde Wollaston, Jacksonian Professor at Cambridge, who, not having conveniences for making experiments with it, in the manner he could wish, was so good as to give it to me" (*Phil. Trans.*, 1798, p. 469).

Coulomb's researches with the torsion balances, and other investigations to which they led, were described by him in papers contributed to the *Mémoires de l'Académie Royale des Sciences* from 1784 onwards. (For an annotated German translation of the more important of these, see Ostwald's *Klassiker*, No. 13.)

The construction and use of the torsion-balance may be ex-

plained by reference to Illustr. 112. A glass cylinder, ABCD, about thirty cms. in height and in diameter, and with its circumference graduated in degrees, is covered by a glass plate perforated by one round hole in the centre and by another at the side. Rising from the central hole, and cemented to the glass plate, is another cylinder about sixty cms. in height, surmounted by a graduated torsion-micrometer op, from which a fine silver wire qP hangs down into the lower cylinder. This wire, which can be turned through measured angles by the torsion-head, supports at its lower extremity a fine horizontal rod consisting of a silk thread or a straw coated with sealing-wax, having at one end of it a small pith-ball, a, and at the other end a paper disc, g, to serve as counterpoise and to damp oscillations. The several constituent parts of the apparatus are shown separately to the right of the illustration. Through the second hole, m, in the glass cover a second pith-ball, equal in size to the first, can be lowered into the vessel on an insulating rod, and brought into the vicinity of the first ball.

The instrument works on the principle, previously discovered by Coulomb, that the twisting of a wire fixed at one end gives rise to a restoring couple proportional to the angle of twist. Hence, upon similarly charging the two pith-balls, the repulsive forces between them, when at various distances apart, could be compared by ~~~ ~~ ~f the graduated head the angles through which the wire

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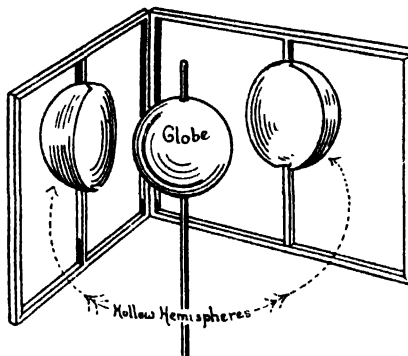ollows: The torsion-
iole torsion-head and

:am; as a unit of force,

It = 0·0531 × 981, or about 52·1 dynes.

degree, would have the same charge with the given conductor, since the charges on equally electrified spheres are proportionate to their diameters. Cavendish was thus able to anticipate the modern measurements of *capacity*, and to measure it in "inches of electricity." He found that the capacities of his condensers (which were discs of tinfoil separated by non-conducting material) depended to some extent on the substance employed to separate the coatings; and he found the numerical ratio of the charges obtained, using given separating substances, to the charges obtained when air was employed, under otherwise similar circumstances. He thus anticipated to some extent Faraday's work on specific inductive capacity. In using condensers of the type described, he found it necessary to allow for the spreading of the charges over the glass. He devised a special apparatus and method for measuring capacities; but his accuracy, though remarkable, must have been impaired by his use of crude electrometers consisting merely of pairs of suspended cork or pith-balls.

In order to compare the conducting powers of various salt and acid solutions of known strengths, Cavendish placed them in glass tubes about a yard long which were corked at each end. Through the corks wires were passed which could be pushed in, or partly withdrawn, so as to vary the effective length of the liquid column. The wires from one end of each of two tubes of solution to be compared were connected to the earthed outsides of a set of Leyden jars, all charged up to the same tension. Cavendish, taking a piece of metal in each hand, then touched with one piece the free wire of one of the tubes, and with the other the knob of one of the jars, when he obtained a shock. He repeated the experiment, making the circuit through the second tube, then again through the first tube, and so on alternately, discharging one of the jars each time until all had been discharged. He judged which tube gave the greater shock, and concluded that that tube offered the smaller resistance. He next varied the length of solution through which the shock had to pass in one of the tubes so as to make the shocks more nearly equal, and went through the process again and again until approximate equality had been reached, when the lengths of the columns could be compared. In like manner Cavendish studied the variation of resistance with temperature. He partially anticipated Ohm's Law by showing that the resistance of a conductor is independent of the strength of the discharge; and he gave the laws according to which the discharge divides itself among a number of conductors in parallel. This work and that described in the next paragraph remained unpublished.

Cavendish proved that the charge upon a conductor resides entirely upon its surface, and he thence deduced the inverse square

law of electrical repulsion. He took his 12·1 inch globe covered with tinfoil, which served also as his standard of capacity, and having insulated it, he enclosed it within two hinged pasteboard hemispheres which, however, did not touch it at any point. He then electrified the hemispheres, connected them momentarily by a wire with the inner globe, and then, having separated the hemispheres,
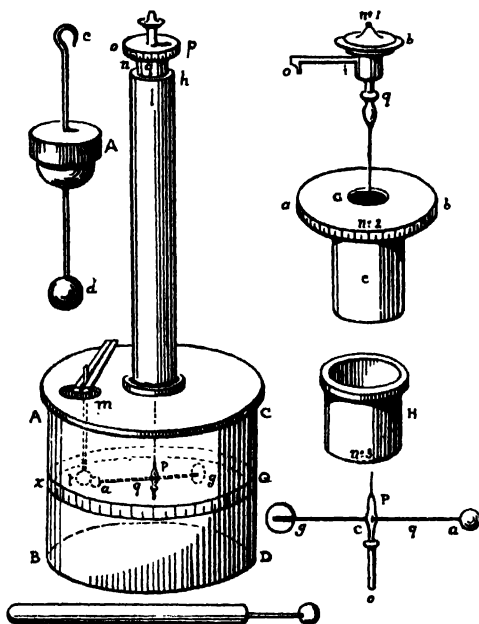

Illustr. 111.—Cavendish's Experiment on the Law of Electric Force

tested the globe with a pith-ball electroscope. He found it to be uncharged, and showed that from this result, if exactly true, the inverse square law of repulsion must follow. From some quantitative experiments with this apparatus he was able to conclude that the electric force must at least vary inversely as some power of the distance lying between $2 + \dfrac{1}{50}$ and $2 - \dfrac{1}{50}$. Maxwell, by a similar experiment, later reduced the limits of uncertainty to $\pm \dfrac{1}{21,600}$.

COULOMB

Charles Augustin Coulomb (1736–1806), a native of Angoulême, began his career as a military engineer, and his scientific interests and enquiries, like those of Von Guericke, grew out of his work. Upon returning in 1776 from a spell of duty at Martinique, he settled in Paris, where he devoted himself to researches dealing chiefly with friction, torsion, and the rigidity of materials, which won him a prize and the membership of the Academy of Sciences. Coulomb was attracted to the problems of electricity and magnetism through the offer of a prize by the Academy for the best method of constructing a ship's compass. It was while investigating this problem, and applying thereto the results which he had obtained on rigidity (especially torsional rigidity), that Coulomb, about 1784, invented his torsion-balance. A torsion-balance was also invented

by John Michell, probably before Coulomb, but there seems to be no evidence that the two men did not arrive at the invention of the instrument independently. Cavendish writes in his*paper on the mean density of the Earth, "Many years. ago, the late Rev. John Michell of this [the Royal] Society contrived a method of determining the density of the Earth, by rendering sensible the attraction of small quantities of matter; but, as he was engaged in other pursuits, he did not complete the apparatus till a short time before

Illustr. 112.—Coulomb's Torsion-Balance

his death, and did not live to make any experiments with it. After his death the apparatus came to the Rev. Francis John Hyde Wollaston, Jacksonian Professor at Cambridge, who, not having conveniences for making experiments with it, in the manner he could wish, was so good as to give it to me" (*Phil. Trans.*, 1798, p. 469).

Coulomb's researches with the torsion balances, and other investigations to which they led, were described by him in papers contributed to the *Mémoires de l'Académie Royale des Sciences* from 1784 onwards. (For an annotated German translation of the more important of these, see Ostwald's *Klassiker*, No. 13.)

The construction, and use of the torsion-balance may be ex-

plained by reference to Illustr. 112. A glass cylinder, ABCD, about thirty cms. in height and in diameter, and with its circumference graduated in degrees, is covered by a glass plate perforated by one round hole in the centre and by another at the side. Rising from the central hole, and cemented to the glass plate, is another cylinder about sixty cms. in height, surmounted by a graduated torsion-micrometer *op*, from which a fine silver wire *q*P hangs down into the lower cylinder. This wire, which can be turned through measured angles by the torsion-head, supports at its lower extremity a fine horizontal rod consisting of a silk thread or a straw coated with sealing-wax, having at one end of it a small pith-ball, *a*, and at the other end a paper disc, *g*, to serve as counterpoise and to damp oscillations. The several constituent parts of the apparatus are shown separately to the right of the illustration. Through the second hole, *m*, in the glass cover a second pith-ball, equal in size to the first, can be lowered into the vessel on an insulating rod, and brought into the vicinity of the first ball.

The instrument works on the principle, previously discovered by Coulomb, that the twisting of a wire fixed at one end gives rise to a restoring couple proportional to the angle of twist. Hence, upon similarly charging the two pith-balls, the repulsive forces between them, when at various distances apart, could be compared by reading off the graduated head the angles through which the wire had to be twisted in order to bring the balls within these distances of each other. For the small angular displacements of the rod *ag* involved in the experiments, the distance between the balls could be taken as proportional to these displacements. The arm of the couple exerted on the wire was also assumed to be constant throughout.

Coulomb's torsion-balance was made very sensitive by the use of an extremely fine wire, a force of only $\dfrac{1}{100,000}$ *grain*[1] weight on the arm (which was $10 \cdot 83$ cms. long) being sufficient to give a deflection of $1°$. When a silk fibre was used, a force of $\dfrac{1}{60,000}$ *grain*[1] weight was found sufficient to turn the system through $360°$.

The most important result of Coulomb's experiments was his proof that "the repulsion between two small similarly electrified spheres varies inversely as the square of the distance apart of their centres." His experimental procedure was as follows: The torsion-micrometer was first set to $0°$, and then the whole torsion-head and

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[1] One French *grain*, as a unit of mass, $= 0 \cdot 0531$ gram; as a unit of force, it $= 0 \cdot 0531 \times 981$, or about $52 \cdot 1$ dynes.

the attached wire were turned round bodily until the suspended ball was opposite the zero division on the lower graduated circle, and under the opening in the glass cover. The second ball was now lowered through the hole and fixed in contact with the suspended ball, and the two balls were then electrified similarly in all respects by contact with a charged conductor. The movable ball was immediately repelled from the fixed one. In one of the experiments described by Coulomb, the beam of the balance was turned through 36°. By twisting the wire, this displacement was *halved* to 18°, when it was found that the torsional angle had increased *fourfold* to 144°. In order to halve this displacement and bring the balls within a *quarter* of their original distance, the torsional angle
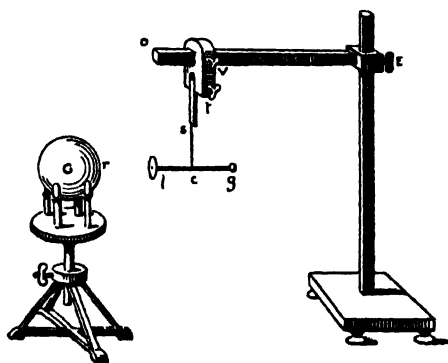
Illustr. 113.—Coulomb's Proof of Law of Electrical Attraction

had to be increased to *sixteen* times its initial value (576°). From such results the above law followed for the case of *repulsion*.

In a second paper, published in 1785, Coulomb described his investigation of the law of electrical *attraction*. Had he used for this purpose the apparatus above described, and charged his balls oppositely, the experiment would have broken down, because when the forces of attraction and torsion were in equilibrium, the system would have been unstable. For any further approach of the balls would have caused the attraction to grow at a greater rate than the torsional resistance, and the balls would have flown together. Hence for this case he employed the following procedure. An insulating needle, having a little gold-leaf disc fixed at one end, was suspended horizontally by a silk thread in the vicinity of a large insulated sphere of conducting material, and level with the centre of this sphere. The sphere and the disc were given opposite charges, the needle was set in horizontal oscillation at various known distances

from the sphere, and the frequency in each case was noted. Assuming the inverse square law to hold good, the charge on the sphere should behave as if concentrated at the centre, and the periods of oscillation of the needle should be as its effective distances

from the sphere. For (period) $\propto \dfrac{1}{\sqrt{(\text{force})}}$, (force) $\propto \dfrac{1}{(\text{distance})^2}$,

$\therefore$ (period) $\propto$ (distance). And this assumption was borne out.

Coulomb corrected for the leakage of charge occurring during his experiments. A special investigation showed that such leakage varied according to the efficiency of the insulation of the charged body, and according to its size, the density of charge upon it, and the humidity of the air.

Coulomb's investigations, by two independent methods analogous to the above, of the law of magnetic force, are described in the section on Magnetism.

Finally, Coulomb investigated the distribution of charges on conductors. He covered an insulated metal sphere (Illustr. 114) with two hemispherical cups fitted with insulating handles. He electrified the whole, and then removed the cups. The sphere was found to be entirely devoid of charge, while the cups were found to be electrified. When the sphere alone was charged and the cups placed upon it in a neutral condition, precisely the same result was found, upon
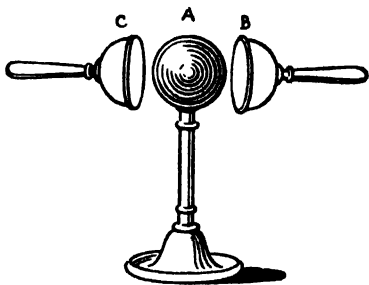
Illustr. 114.—Coulomb's Experiments with Conductors

removing them, as in the first experiment. A more refined experiment of this nature had already been performed by Cavendish, but this was probably unknown to Coulomb. The two fundamental laws of the distribution of charge were formulated by Coulomb as follows: (1) Electricity distributes itself over all conductors to which it is communicated, according to their shapes, without showing a preference for one over another. (2) In an electrified conductor the charge distributes itself over the surface without penetrating the interior. Coulomb, like Cavendish, recognized this latter property as a consequence of the mutual repulsion of the elements of charge according to the inverse square law, of which in fact it provides the most exact confirmation.

With Coulomb the first period in the development of electrical science comes to an end. His labours brought electrostatics to a high degree of completeness. It is true he took no account of the medium

through which electric forces act, and he studied the interactions of charged bodies in air only. The conception of a *dielectric* was characteristic of the later stage in the development of the science, initiated by Faraday. For Coulomb, electrical attractions and repulsions were forces acting at a distance, instantaneously and through empty space, as gravity was conceived to act by the followers of Newton. This, however, by no means impairs the value of Coulomb's contributions, which claimed only to provide precise measurements, excluding all speculations as to the nature of electricity. As such, they were characteristic of the work of many French physicists, and they formed the foundations upon which the following generation could construct a mathematical theory of electrical phenomena. This task was fulfilled, with the aid of the higher analysis, and especially of the theory of potential, from the first decades of the nineteenth century onward.

(See G. R. Sharp: *The Physical Work of Coulomb*, 1936, M.Sc. Dissertation, Library of the University of London.)

D. ELECTROMETERS

The electricians of the eighteenth century soon felt the need of instruments for detecting, and so far as possible measuring, electric charges. This need arose especially in connection with such problems as those of ascertaining the presence and nature of induced charges, of detecting atmospheric electricity, of constructing frictional series, of standardizing the charges given to Leyden jars, and so forth. The invention and progressive improvement of electroscopes and electrometers played an important part in the transformation of electrostatics into an exact science; and eventually it made possible Volta's investigation of the minute charges arising from the contact of dissimilar conductors, which led to the invention of the voltaic pile, and the production of the electric current. Closely connected with the development of such instruments was the invention of a number of auxiliary devices intended for the purpose of multiplying any exceptionally minute charge so as to render it capable of appreciably affecting an electroscope.

At the close of the sixteenth century William Gilbert had employed a pivoted metal pointer to demonstrate the presence of electric charges upon non-conductors that had been subjected to friction. The primitive electroscopes employed at the beginning of the eighteenth century usually consisted of strips of brass foil, or of threads, suspended from wires or from glass tubes. Thus, in order to investigate the attraction exerted by an electrified glass globe

upon light bodies in its vicinity, Hauksbee suspended a number of woollen threads from a hoop of wire concentric with the globe; when the latter was rotated and electrified by friction, the threads set themselves with their free ends pointing straight towards the centre of the globe (*Physico-Mechanical Experiments*, 1709). The electroscopic methods adopted by Stephen Gray some twenty years later were very similar to those of Hauksbee. Following Du Fay's discovery of electrical repulsion, however, a new type of electroscope was introduced. In its primitive form, as described by Gray's friend and collaborator, Granville Wheler (*Phil. Trans.*, 1739, p. 98), it consisted of two pieces of thread hanging side by side from a silk line; these diverged upon the approach of a charged body. Wheler attached feathers to the ends of the threads. Later, Waitz suggested using two pieces of metal suspended side by side by silk threads, so as, from their divergence when charged, to compare their mutual
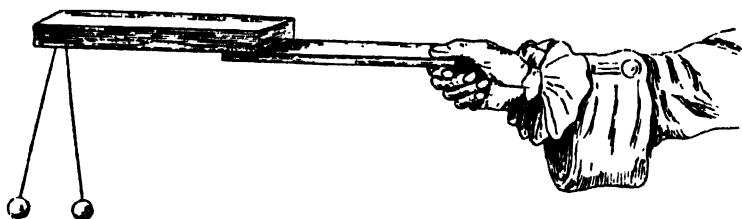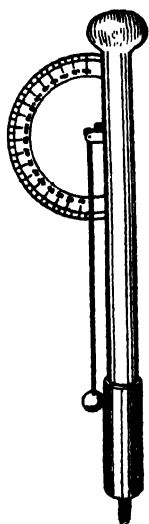

Illustr. 115.—Canton's Pith-Ball Electrometer

repulsion with the force of gravity. (*Abhandlung von der Elektricität*, Berlin, 1745). Nollet used to measure the divergence of such pairs of threads by casting their shadows upon a screen, and using a protractor (*Mém. de l'Acad. Roy. des Sc.*, Paris, Année 1747). John Canton, in his experiments on induction and atmospheric electricity, employed a pair of linen threads about six inches long, hanging side by side from a wire and each supporting a ball of cork or elder pith about the size of a pea (*Phil. Trans.*, 1753, p. 350, and 1754, p. 780). According to Priestley, the suspended balls were sometimes kept in a narrow box with a sliding lid; this served as a handle by which to hold the instrument when it was in use (Illustr 115).

It was desirable to standardize the charges imparted to Leyden jars, especially when they were to be used to give shocks as a form of medical treatment. Several methods were suggested by Thomas Lane, whose original procedure (*Phil. Trans.*, 1767, p. 451) depended essentially upon the use of a spark-gap, the separation of the terminals of which could be varied with great refinement by means of a

screw. The inside of a Leyden jar was charged directly from an electrical machine, while the outside was earthed (see Illustr. 100, f. p. 220). The patient held in one hand an earthed wire, and in the other an insulated metal part separated from the knob of the jar by the spark-gap. When the jar had acquired a sufficiently powerful charge, a spark crossed the gap, and the patient received a shock the intensity of which could be regulated by varying the separation of the terminals between which the spark passed. An instrument especially suited for indicating the intensity of the charge on a Leyden jar, or battery of jars, was the *quadrant electrometer* (Illustr. 116), invented by William Henly in 1770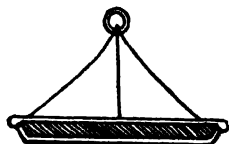, and described by Priestley in a letter to Franklin which was later published in the *Philosophical Transactions* (1772, p. 359). In this instrument a cork ball was hung by a light rod from the centre of a graduated semicircle or quadrant over which the rod turned. The whole was attached to an ivory or boxwood stem which could be screwed into the prime conductor of a machine or into the knob of a Leyden jar. The charge thereon was measured (in arbitrary fashion) by the deflection of the rod supporting the ball. Henly used this instrument to compare the conducting powers of various metals, by noting what charge, measured by the electrometer, was required just to melt one inch of wire of each metal (*Phil. Trans.*, 1774, p. 389). Henly also directed his attention to the problem of devising an electroscope adapted to investigating the electrical state of the atmosphere. Improving upon a suggestion made by Nairne for screening the instrument from draughts, Henly recommended suspending a pair of pith-balls side by side in a perforated bottle by threads hanging from a collecting wire which projected several inches above the cork of the bottle (*Canton MSS.*, II, p. 94). Several other adaptations of Canton's pith-ball electroscope were suggested by Henly's friend Tiberius Cavallo (1749–1809), an Italian electrician who spent nearly all his life in England. Cavallo's most celebrated electrometer (invented in 1777, and described in *Phil. Trans.*, 1780, p. 14) was the first such instrument in general use to have its movable parts completely enclosed in a glass vessel (Illustr. 117, Fig. 2). The place of the linen threads was taken by fine silver wires, supporting conical pieces of cork and connected, by an insulated wire passing up through the neck of the bottle, to a brass cap on the top of the instrument. Earthed strips of tinfoil were attached to the inside of the bottle on each

Illustr. 116.—
Henly's Quadrant
Electrometer
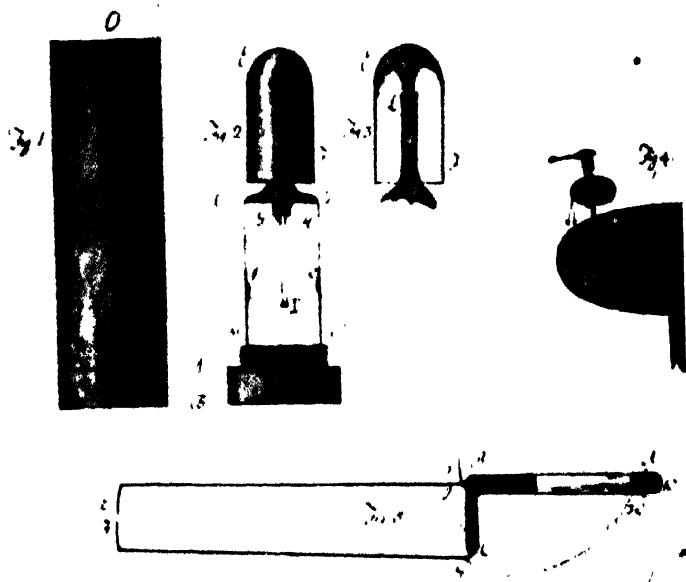
Cavallo's Bottle Electrometer
(By courtesy of the Royal Society, London)

side, for the purpose (it was stated) of earthing the corks when these were made to diverge sufficiently widely to come into contact with the foil (as was done whenever it was desired to charge the corks by induction). The whole instrument fitted into a case about three inches in height.

The electroscopes employed by Cavendish in his researches differed little in general from those of Canton. However, in the course of his experiments on the properties of the electric ray, or *torpedo*, Cavendish made use of "a very exact electrometer," which he describes as follows: "The electrometer I used consisted of two straws, 10 inches long, hanging parallel to each other, and turning at one end on steel pins as centres, with cork balls about a quarter of an

Illustr. 118.—Volta's Electrophorus

inch diameter fixed on the other end. The way by which I estimated the divergence of these balls, was by seeing whether they appeared to coincide with parallel lines placed behind them at about ten inches distance; taking care to hold my eye always at the same distance from the balls and not less than thirty inches off. To make the straws conduct better they were gilded, which causes them to be more regular in their effect. This electrometer is very accurate; but can be used only when the electricity is very weak."

Cavallo's electroscope was modified in several minor respects by De Saussure. A more radical reform in methods of estimating electric charges was achieved by Volta, who was already known to electricians as the inventor of the electrophorus (*elettroforo perpetuo*), an account of which he had given to Priestley in 1775. This device, shown in Illustr. 118, consisted of a metal sole containing a cake of non-conducting material, and a metal cover suspended from a ring by insulating silk threads, or fitted with an insulating handle. The cake was excited by friction, and the metal plate was then placed upon it, earthed by contact with the finger or with the metal sides of the sole, and then removed carrying with it an induced charge which could be delivered to a conductor. The process could be repeated many times before the cake needed to be charged up again. Electrophori of considerable size (up to 7 feet in diameter) were constructed towards the close of the eighteenth century. Volta developed the instrument into a form to which he gave the name of a *condenser*; and

Illustr. 119.— Volta's Condenser

it was he who laid the foundation for the subsequent evolution of this type of instrument. Volta's condenser was essentially an electrophorus having a thin layer of resin in place of the cake, and it finally took the form shown in Illustr. 119. It was used in detecting minute charges or degrees of electrification (*Of the Method of rendering very sensible the weakest natural or artificial Electricity*, London, 1782). It was with an instrument of this kind that Volta, Laplace, and Lavoisier demonstrated that electricity is liberated in small quantities by the combustion of coals, the solution of iron filings in vitriol, and the evaporation of water. In order to test the minute charges involved in these experiments, Cavallo's electroscope was at first used. In two letters which he wrote to Lichtenberg in July 1787, however, Volta made a critical survey of existing methods of estimating electric charges, and he gave an account of his own newly invented electrometer (*Opere*, 1816, Tome I, Parte II, *Meteorologia Elettrica*). This instrument consisted essentially of two fine straws about 2 inches in length, suspended close together side by side from rings of silver wire so as to diverge readily when charged. The whole was enclosed in a square glass vessel with a circular scale pasted on one side; and the silver wires communicated with an external metal cap, for which one of Volta's condensers was later substituted to form a condensing electroscope. Volta claimed to have established experimentally that the divergence of the straws was proportional, over a wide range, to the charge given to the electrometer.

Volta was anxious to establish an absolute electrometric scale, independent of the peculiarities of any particular kind of measuring instrument. Already about the middle of the eighteenth century several writers had suggested measuring the strength of the electrification of a body by allowing it to pull down one pan of a balance by its attractive force, and finding the weight which must be put in the other pan in order just to counterbalance this attraction. Such a suggestion was made in a letter from an unknown writer to John Ellicott, the instrument-maker, which was published in the *Philosophical Transactions* (1746, p. 96). This principle was actually employed in Gralath's electrometer: it consisted of a balance one pan of which was situated perpendicularly over the upper end of an iron rod whose lower end rested upon a table which could be raised or lowered at will by means of a screw. The rod was electrified by means of a frictional machine connected to it by wires. The attraction of the electrified rod upon the one pan was counterpoised (and measured) by weights placed in the other pan. Gralath observed and recorded how the weights required in order to produce equilibrium varied with alterations in the distance of the pan from the attracting rod and in the distance of the electrical machine from
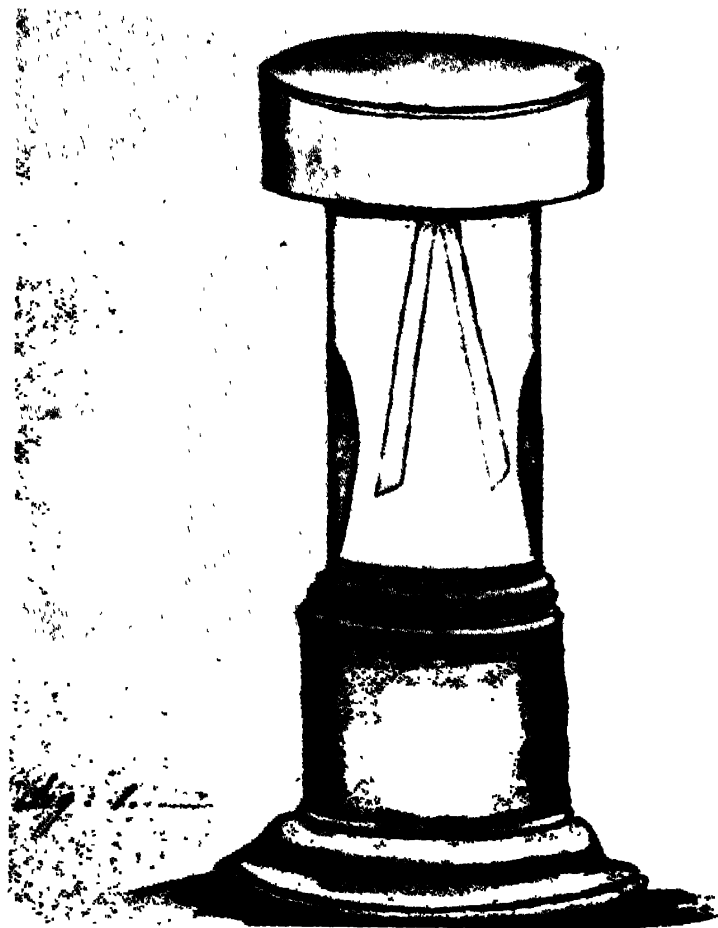
Bennet's Gold-leaf Electrometer

the rod (*Versuche und Abhandlungen der naturforschenden Gesellschaft in Danzig*, I, 1747, pp. 506–34; Gralath's account of his instrument is in Section IV of his paper). Volta sought to standardize the measurement of electrification on rather similar lines, as he explained to Lichtenberg in the second of his letters of 1787. He placed the charged body in conducting communication with a metal disc 5 inches in diameter, which was hung by silk threads from the beam of a balance so as to lie about 2 inches above a horizontal earthed plate. He found what weights were needed to counterpoise the attraction upon the disc when this was connected to a Leyden jar to which a graduated series of charges was then imparted; and he sought to correlate these weights with the corresponding deflections shown by a Henly's quadrant electrometer attached to the jar.

Another interesting development from the ideas of Cavallo was the well-known gold-leaf electrometer of Abraham Bennet (1750–99), curate of Wirksworth, in Derbyshire. This instrument (Illustr. 120), which is still frequently employed for elementary purposes, consisted in its original form of two tapering strips of gold-leaf enclosed in a glass vessel 5 inches high; they were suspended from an ivory peg communicating by a tin tube with the insulated metal cap of the vessel. Two strips of tinfoil were fastened with varnish to the sides of the vessel on its inner surface opposite to the gold leaves, and the divergence of the latter upon charging the cap was measured on a graduated paper scale. Bennet found his instrument sufficiently sensitive to detect the electrification due to the evaporation of water in a metal vessel placed on the cap (*Phil. Trans.*, 1787, pp. 26, 32). Bennet sought to improve upon Volta's condensing electrometer with his "doubler of electricity" (*Phil. Trans.*, 1787, p. 288). This instrument was intended for magnifying charges which would otherwise have been too minute for detection. It consisted of a gold-leaf electroscope, and two brass plates varnished on their under sides and provided with insulating handles. Supposing a small charge (say, a positive one) to have been communicated to the electroscope, and calling the two plates B and C, the procedure was as follows. The plate B was laid on the cap of the electroscope and earthed; it thus acquired an induced negative charge nearly equal to that on the instrument. B was then removed by its handle, and C was laid on B (varnished side downwards) and earthed; C thus acquired an induced positive charge. B was replaced on the cap and earthed; C was then connected with the cap, to which it gave up nearly the whole of its positive charge; B was next removed with nearly twice the negative charge which it had in the first instance; and the cycle was repeated until the electroscope showed a sensible divergence.

Several attempts were made to devise a mechanical method of

performing the operations involved in Bennet's doubling process. The most notable of these was the instrument known as "Nicholson's Doubler," after its inventor William Nicholson, who described it to Sir Joseph Banks early in 1788 (*Phil. Trans.*, 1788, p. 403).

Such devices for multiplying charges mechanically soon came to be treated as little more than toys. They may, however, be regarded as the precursors of the modern Influence Machines, working by induction, which, since the latter part of the nineteenth century, have superseded frictional machines as sources of electrification.

(See W. Cameron Walker: "The Detection and Estimation of Electric Charges in the Eighteenth Century," in *Annals of Science*. Vol. 1, 1936, pp. 66–100.)

E. Galvanism

While the electrification of certain substances by friction had been familiar from antiquity, the physicists of the eighteenth century showed that electricity could also be produced by heating certain crystals, and that it could be collected from the atmosphere. They further demonstrated the electrical nature of the shock of the torpedo. But more important than any of these was the discovery of a fifth source of electrification in the phenomena of *galvanism* or *contact electricity*, which were observed towards the close of the eighteenth century. The fuller exploration of these phenomena, and their theoretical interpretation, may be regarded as the greatest achievement of nineteenth-century physics.

SULZER

That the mere contact of two metals was capable under certain circumstances of producing a peculiar sensation, was first noticed about 1750 by J. G. Sulzer, a German Professor of Mathematics, though it was not until later that this effect was connected with
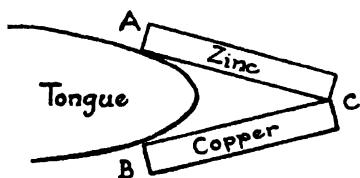
Illustr. 121.—Contact Electricity

electricity (*Theorie der angenehmen und unangenehmen Empfindungen*, Berlin, 1762). Sulzer happened to put the tip of his tongue between pieces of two different metals whose edges were in contact; and he noticed a pungent sensation, reminding him of the taste of green vitriol, which the metals, when placed separately upon the tongue, could not produce. This experiment, which is sketched in Illustr. 121, can easily be repeated upon inserting the tongue between well-scoured strips of zinc (A) and copper (B) placed in contact along their edges

(C). Sulzer thought it unlikely that any decomposition occurred upon bringing the metals together. He supposed rather that the contact of the metals set up a vibratory motion in their particles which excited the nerves of taste. In a later form of this experiment, a beaker of tin or zinc was mounted upon a silver foot and filled with water. Upon dipping the tip of the tongue into the water the latter was found to be entirely tasteless so long as there was no contact with the silver foot. But when this foot was simultaneously grasped between the moistened hands, a distinct taste was perceived. Sulzer's discovery remained an isolated observation for the time being, and as is usual in such cases, it was unheeded and eventually forgotten until the further development of the science made it necessary to revert to this discovery.
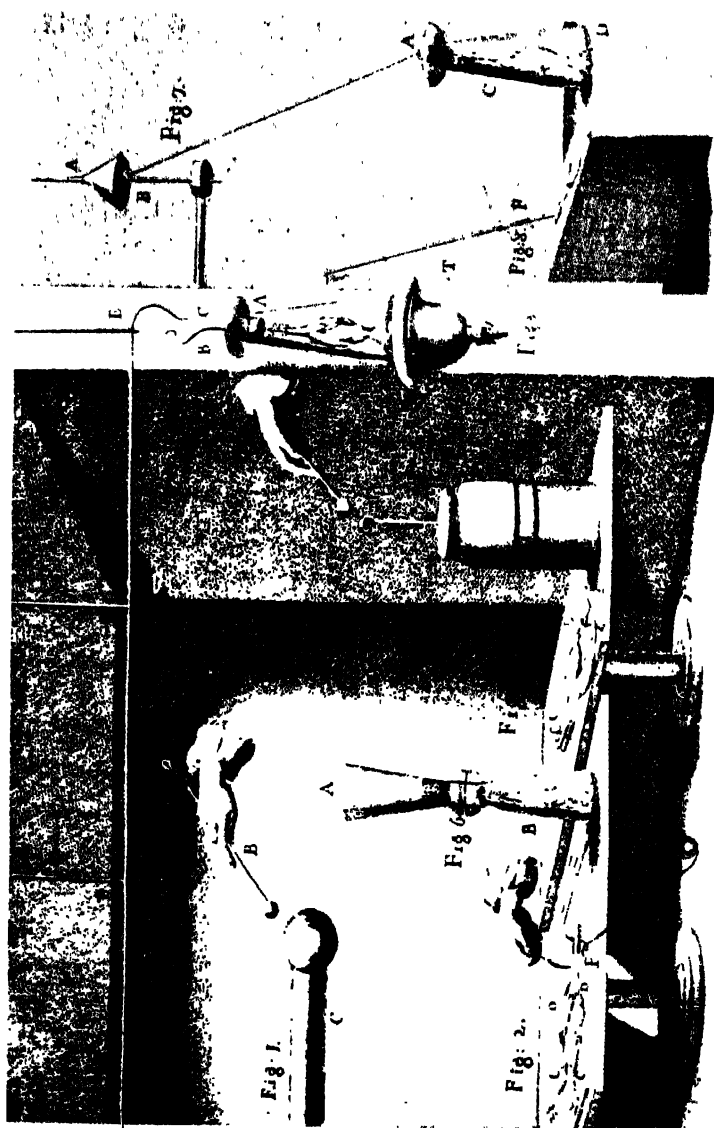

GALVANI

The real investigation of contact electricity arose from the chance observation that a freshly prepared frog's leg was thrown into convulsions whenever an electrical discharge occurred in its neighbourhood while it was in conducting communication with the earth. This behaviour in a frog's leg was noticed about 1780 by the Italian Luigi Galvani (1737–98), Professor of Anatomy at the University of Bologna. Galvani kept a diary of his epoch-making experiments, and he later published an account of them under the title *De viribus electricitatis in motu musculari commentarius* (1791), upon which our account is based. (For an annotated German translation, see Ostwald's *Klassiker*, No. 52.)

It had already long been known that muscular convulsions could be produced in dead animals under the influence of direct electric discharges through the body, and that a torpedo could produce motion in dead fish. What surprised Galvani was the fact that the convulsions which he observed occurred in the absence of any connection between the electrical machine and the prepared frog. Galvani prepared a frog as shown in Illustr. 122 (Fig. 2, on the extreme left), and laid it upon a table on which an electrical machine was standing. As one of his assistants happened lightly to touch the crural nerve, DD, with the point of a knife, all the muscles at the joints contracted, as if affected by severe cramp. This happened while a spark was being taken from the conductor of the machine; without the spark, nothing occurred. The experiment likewise broke down when the knife was held by its bone haft without touching the blade. This suggested an electrical element in the phenomenon, as Galvani confirmed by touching the nerves alternately with a glass cylinder and with an iron cylinder: only when the latter was used

did the sparks produce any effect. So far, however, the phenomena had nothing to do with contact electricity, but were produced by a return-shock, the distribution of electricity in the frog's leg due to the charge on the machine being altered at the instant of discharge. For this electrical distribution, and its equalization by the discharge, to be appreciable at a distance from the conductor of the machine, it was necessary to place the leg in conducting communication with the earth. In Galvani's experiments this was first done by the chance contact of the knife with the nerve, but thereafter intentionally by bringing the leg into contact with a conductor. Similar effects were observed when a Leyden jar was discharged, instead of the machine, and when a living animal was used instead of the dead preparation. The bodies of warm-blooded animals could be made to give similar convulsions, but they lost the property sooner than did the cold-blooded frog.

The amazement which seized Galvani at these observations impelled him to continue his experiments, and was the prelude to an almost endless series of important discoveries in this new domain. Having demonstrated the effect of a discharge upon a frog's leg in the neighbourhood of the electric machine, Galvani next tried to ascertain whether a similar effect could be produced by the influence of atmospheric electricity, lightning here playing the part of the artificial discharge of the electric machine or Leyden jar. His experiments on this problem are described in the second part of his book. Frogs prepared as above described, together with legs of warm-blooded animals, were attached by the nerves to long iron wires during a thunderstorm, the feet being connected to earth by similar wires. Galvani's expectations were not disappointed. Simultaneously with a flash of lightning, the muscles were thrown into lively convulsions. Having thus tested the effects of the electricity associated with thunderstorms, Galvani wished to investigate the power of the electricity permanently present in the atmosphere to excite the mysterious convulsions, and he was thus led to the discoveries described in the third part of his book.

He had noticed that prepared frogs attached to an iron trellis, and having brass hooks driven into the spinal marrow, showed occasional convulsions not only during thunder but even in fine weather. He thought that the cause of these movements must be sought in alterations in the electrical state of the atmosphere, and he accordingly observed the animals which he had pinned up, at various hours throughout the day. But movements of the muscles were only seldom noticeable, and finally, tired of waiting, Galvani pressed the brass hooks against the iron trellis. Immediately he observed repeated convulsions which he was at first inclined to attribute to atmospheric

Galvani's Experiments (1)

Illustr. 123
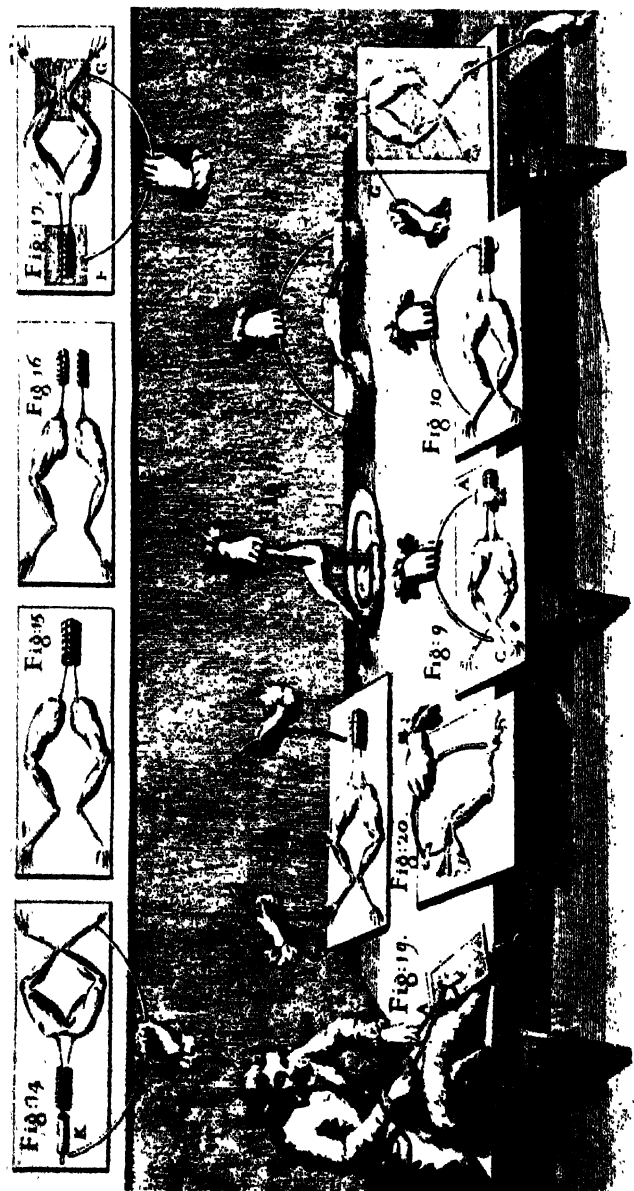
Galvani's Experiments (2)

electricity. But when he brought one of the preparations indoors, laid it upon an iron plate, and pressed the brass hooks in the spinal marrow against the iron plate, he again observed similar convulsions.

Galvani now recognized that he was dealing with an entirely new and unexpected phenomenon, having no relation whatever to changes in the atmospheric electricity. He varied the experiment by placing the frog upon a non-conducting glass plate and joining the brass hook to the feet of the animal. If the connection was made by means of another metal, convulsions occurred, but they were absent when the same metal, or a non-conducting substance, was employed. Some of the variations of this fundamental experiment devised by Galvani are shown in Illustr. 123, taken from his book. Of particular interest is the species of electrical pendulum shown at Fig. 11. This was constructed by holding up a prepared frog by one leg, with the brass hook through the marrow in contact with a silver plate, and the other leg free to slide over the plate. As soon as this leg touched the plate, the muscles were contracted so that the leg rose in the air. When the circuit was thus broken, the muscles relaxed, bringing the leg once more into contact with the plate. Thereupon another convulsion took place, the leg was again raised, and so the process continued, the leg oscillating somewhat after the manner of a pendulum.

There were only two possible explanations of this remarkable phenomenon. Either it was due to electricity in the animal organism, or it involved some electrical process depending upon the contact of the metals, the frog's leg merely playing the part of a sensitive electroscope. Galvani declared for the former opinion, conceiving all these phenomena as proofs of the existence of an animal electricity. This was supposed to flow from the brain through the nerves and so to the muscle, which he likened to a Leyden jar, supposing the surface and the interior of a muscle to be oppositely charged. Upon bringing into conducting communication the nerves and the surface of the muscle (which respectively corresponded to the inside and the outside of the jar) a discharge took place of which he conceived the muscular contraction to be a consequence.

Galvani's experiments and his theory, which at first received general acceptance, naturally aroused the greatest interest among physicists, physiologists, and medical men, who were all anxious to procure frogs and pieces of dissimilar metals and to repeat the experiments for themselves.

Galvani's scientific activity culminated in the publication of his *Commentarius*, and thereafter the lead in this new field of enquiry was assumed by his countryman Volta. Galvani merely continued to defend his theory of "animal electricity" against Volta's assaults

upon it; but he gradually sank into a state of deep depression, and died without living to see his theory finally overthrown by Volta's invention of the electric pile.

VOLTA

Alessandro Volta (1745–1827) was a native of Como, where he became Professor of Physics when not yet 30 years of age. Five years later he was called to a similar post at the University of Pavia, and in 1815 he became director of the Philosophical Faculty at Padua, where he worked until 1819. The closing years of his life were spent in retirement. At the beginning of his career Volta devoted himself to studying the properties of gases, but even before the publication of Galvani's results in 1791 he had made important contributions to electrical science. To this early period belongs his invention of the electrophorus and condenser, already described, which he used in conjunction with his straw-electrometer as a means of detecting minute quantities of electricity. This instrument was to be of the greatest value in Volta's subsequent investigations on contact electricity, and it early won him the Fellowship and the Copley Medal of the Royal Society. Volta's experiments and speculations were largely set forth in letters to his friends. (For annotated German translations of the most important of these, see Ostwald's *Klassiker*, Nos. 114 and 118.)

Volta began by repeating and confirming Galvani's experiments. At first he was convinced of the correctness of Galvani's views concerning "animal electricity." He supposed that the muscular convulsions observed by Galvani must proceed from a disproportion between the electricity of the muscle and that of the nerve, the metallic connection merely serving to restore equilibrium. Some years later, however, he recognized that there could be no question of likening the muscle to the coating of a Leyden jar, since the frog's leg showed convulsions even if the equalization of charges took place entirely through the nerves, the muscles remaining wholly outside the conducting circuit. After the manner of Sulzer's experiment Volta succeeded, by applying pieces of two different metals to the mouth and the eye, in producing not merely a sensation of taste but also a sensation of light. For this fundamental experiment, which proved that an electric discharge could not only cause muscular convulsions but could excite the sensory nerves, Volta proceeded as follows. He placed a broad piece of tinfoil on the tip of the tongue, and a silver coin on the back of the tongue. Upon connecting the two metals by means of a copper wire, he was aware of a strong sour taste. Volta obtained the same result without the copper wire

upon substituting for the coin a silver spoon which was laid on the back of the tongue with its handle touching the tinfoil lying on the tip. He obtained a light-sensation galvanically by making a connection between the forehead and the palate through dissimilar metals, a bright flash being perceived at the instant of contact. From such investigations the conviction steadily grew on Volta that the metals did not merely play the part of conductors, but actually generated the electricity themselves. Accordingly, in his description of these physiological researches, Volta, about 1792, modified his original views. It was clear, he thought, that in these experiments the nerves were merely stimulated, and that the cause of the electric current which produced this stimulation was to be sought in the metals themselves. "They are," he wrote, "in a real sense the exciters of electricity, while the nerves themselves are passive." About this time Volta discovered that charcoal could be used in place of a metal as a conductor and exciter of electricity in galvanic experiments.

In a letter written in 1794, Volta came out as an opponent of the theory of animal electricity, for which he henceforward substituted the term *metallic electricity*. The entire effect, he held, proceeded from the metals when these were brought into contact with any moist body, thereby setting the electricity in circulation. If the current passed through nerves which still possessed some remnant of vitality, the muscles controlled by those nerves would thereby be thrown into convulsions. Such movements, and the sensations of taste and sight above described, were found by Volta, in the course of uninterrupted and laborious researches, to differ considerably according to the nature of the metals employed. The effects were the more vigorous the farther apart the substances lay in the following series, which Volta drew up in 1794: zinc, tin, lead, iron, copper, platinum, gold, silver, graphite, charcoal. This first attempt to construct what would now be called an electrochemical series was soon enlarged by the addition of many other members including such minerals as pyrites, galena, copper-ore, etc.

Volta strove henceforward to dispense altogether with the participation of nerves and muscles in galvanic phenomena. He brought pairs of metals into contact with all sorts of moist substances such as paper, cloth, etc. In order to demonstrate unmistakably the resulting equalization of charge which had hitherto shown itself in the muscular convulsions, Volta employed his condenser to magnify the effects of the smallest quantities of electricity. His preliminary trials with this instrument paved the way for his well-known and fundamental experiment on contact electricity, which showed that the mere momentary contact of two different metals caused them to

become oppositely charged without the mediation of any moist substance, whether animal or not. Nothing further was required in the experiment than plates of various metals furnished with insulating handles, a condenser, and a delicate gold-leaf electrometer with which to test and measure the charges produced on the metals by mutual contact. Thus, for example, a zinc and a copper disc, after contact, were both found to be charged, the former positively, the latter negatively. Again, after contact with tin or iron, copper was negatively though much less strongly charged, the former metals becoming positive. But upon being brought into contact with gold or silver the copper acquired a positive charge, the precious metals becoming negatively electrified. Volta described this fundamental experiment in a letter in 1797, in which he went on to stress how remarkable it was to obtain such a considerable quantity of electricity by the mere contact of different metals, and how all the experts to whom he had shown his experiment had been amazed at it. In order to ascertain the signs of the various contact-charges, Volta communicated them to his electrometer, and then brought up to it rubbed rods of glass and resin and observed which made the divergence of the leaves increase and which made it decrease. By numerous variations of his fundamental experiment, Volta was led to arrange the substances tested as follows:

+	
zinc	copper
lead	silver
tin	gold
iron	graphite

the substances being so arranged that, upon bringing any two of them into contact, the earlier in the list became positively, and the later negatively charged. Further, measurements with the straw-electrometer showed that the separation of electricities due to the contact of any two members was so much the greater the farther apart the substances lay in the list. Thus, for the first four members of the series, the following differences were found:

$$\text{zinc/lead} = 5$$
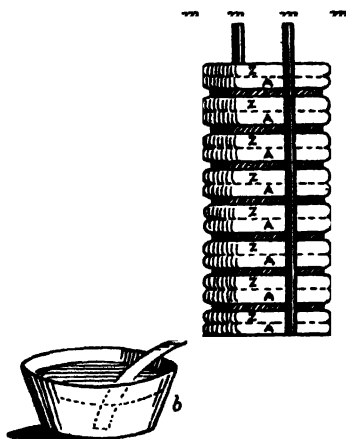$$\text{lead/tin} = 1$$
$$\text{tin/iron} = 3$$

while for zinc/iron, the difference was $9(= 5 + 1 + 3)$. This result helped to establish the "law of successive contacts," namely, that the electrical separation for any two members of the series is equal to the sum of the separations between all the intermediate members, so that in a closed circuit made up of various metals, the separations

would cancel out round the circuit. Ritter recognized that this electro-chemical series also represented the arrangement of the metals in such an order that each displaces the following ones from their compounds in solution, and is itself displaced by the preceding ones. He was thus led to assume that galvanism has its cause in chemical processes—a view which ran counter to Volta's contact-theory, and which did not prevail until it later found support in Faraday's researches.

On the basis of his investigations Volta had at first assumed that the exciting force of electricity resided exclusively at the points of contact of the metals, and that the animal or other fluids served merely as conductors. Further investigation taught him, however, that an exciting or electro-motive force occurs also when there is contact between a metal and a fluid. Insulated discs of silver, tin, zinc, etc., were brought into contact with moist wood, paper, or tiles, and upon being removed were found to be negatively electrified. The metals he called electro-motors of the first class, and the liquids, which could not be included in the electro-chemical series, electro-motors or conductors of the second class. He confessed himself unable, however, to explain how the contact between

Illustr. 124.—Volta's First Pile

members of these two classes of substances could give rise to electrification, as it was generally observed to do.

Volta showed that in a circuit composed entirely of electro-motors of the first class (metals) no movement of electricity, no current, took place. But he further showed that such a current was produced when two electro-motors of the first class were connected with an intermediate moist conductor, and with each other either directly or by means of a third conductor, so as to form a conducting circuit. Such a combination was called a galvanic element. Volta multiplied the effects of such a single element by combining a large number of them to form a "pile."

The earliest account of this invention, which has proved of first-class importance, was given by Volta in a letter to Sir Joseph Banks, President of the Royal Society, dated March 20, 1800 (*Phil. Trans.*,

1800, p. 403). He relates therein that he had succeeded, in the course of his experiments on contact electricity, in constructing a new apparatus. It possesses, he says, the properties of the Leyden jar in a very weak degree, but on the other hand it has the great advantage over the jar that it does not need to be charged with electricity from outside, but acts spontaneously when touched in the proper way. He compares the action and arrangement of the apparatus to the electric organ of the torpedo. The first of Volta's piles is shown in Illustr. 124. He describes its construction as follows: "Thirty, forty, sixty, or more pieces of copper, or better of silver, each applied to a piece of tin, or, much better still, of zinc; and an equal number
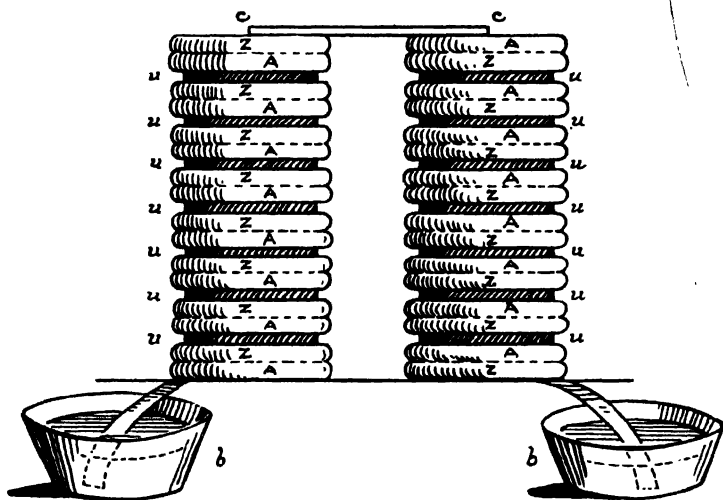
Illustr. 125.—Volta's Second Pile

of layers of water or of some other liquid which may be a better conductor than simple water, such as brine, lye, or pieces of card, skin, etc., well soaked in these liquids, such layers being interposed between each pair or combination of two different metals,—one such alternate series, and always in the same order, of these three kinds of conductors, is all that constitutes my new instrument" (*Phil. Trans.*, 1800, p. 403).

Besides obtaining a slight shock upon touching the uppermost plate and dipping the other hand into the vessel *b* so as to complete the circuit, the action of the apparatus upon the nerves of taste, sight, and hearing, could also be demonstrated. Upon combining a large number of plates Volta was obliged either to place supports round the pile or to divide it up into several parts (Illustr. 125).

One of the drawbacks to the pile was that the discs of metal, by pressing upon the discs of cloth, eventually caused the fluid contained in the latter to flow over the whole of the pile, and to put it out of action. Volta therefore devised an arrangement which got over this difficulty. He set up a row of beakers, made of glass or of some other non-metallic material, and half-filled them with brine or lye. He then connected them up in series, as shown in Illustr. 126, by inserting a plate of copper or silvered copper (A), and a plate of tin or zinc (Z), in each vessel, and connecting each copper plate by soldering to the zinc plate in the vessel next in order. "A series of thirty, forty, sixty, of these cups, connected in this manner and arranged either in a straight line, or in a curve of any shape, forms the whole of this new apparatus, which in principle and in regard to the substances employed is the same as that in the form of a column." In order to obtain a shock from the *couronne de tasses*, as Volta, writing in French to Banks, called this apparatus, it sufficed to dip one hand in one of the beakers and one finger of the other hand in a second beaker. The shock was stronger in proportion as the two

Illustr. 126.—Volta's Crown of Cups

beakers were more widely separated in the series. Volta thus obtained the most severe shock when he put his hands into the first and the last beakers in the series. Besides giving momentary shocks, a crown of forty or fifty cups could be used to stimulate the organs of taste, sight, and hearing, and the sense of touch. Volta describes in detail his sensations upon allowing the current to pass steadily through his body for some time. The fact that the same electrical stimulus excited in each sense-organ the sensation characteristic of that organ was of the greatest significance for the physiology of the sense-organs. It later led Joannes Müller to put forward his doctrine of the specific energies of these organs.

In his experiments with the crown of cups Volta noticed that the strength of the current varied according to the concentration of the salt-solution employed, and that the best results were obtained when the water and surrounding air were warm. He also studied the way in which the current divided itself among parallel circuits, but he seems to have been unaware of the chemical processes attending the passage of the current, or to have taken no interest in them.

The invention of the Voltaic pile excited the greatest interest in England and France. Napoleon, in 1801, invited Volta to Paris, where he lectured and was loaded with honours.

CARLISLE AND NICHOLSON

The first man in England to construct an electric pile according to Volta's directions appears to have been Sir Anthony Carlisle (1768–1840), a London surgeon and Professor of Anatomy. It came about in this way. Sir Joseph Banks showed Carlisle the first part of Volta's letter on the pile at the end of April 1800; and Carlisle immediately set about constructing such an apparatus for himself. Nicholson writes: "On the 30th of April, Mr. Carlisle had provided a pile consisting of seventeen half-crowns, with a like number of pieces of zinc, and of pasteboard, soaked in salt water. These were arranged in the order of silver, zinc, card, etc. . . . the silver was . . . under the zinc." Experiments were made with a Bennet electrometer and doubler to prove that the shock obtained from the pile was an electric phenomenon, and they showed that the silver end was negatively, and the zinc end positively, charged. In the course of the subsequent experiments, "the contacts being made sure by placing a drop of water upon the upper plate, Mr. Carlisle observed a disengagement of gas round the touching wire. This gas, though very minute in quantity, evidently seemed to me to have the smell afforded by hydrogen when the wire of communication was steel. This, with some other facts, led me to propose to break the circuit by the substitution of a tube of water between two wires. On the 2nd of May we, therefore, inserted a brass wire through each of two corks inserted in a glass tube of half an inch internal diameter. The tube was filled with New River water, and the distance between the points of the wires in the water was one inch and three quarters. This compound discharger was applied so that the external ends of its wire were in contact with the two extreme plates of a pile of thirty-six half-crowns with the correspondent pieces of zinc and pasteboard. A fine stream of minute bubbles immediately began to flow from the point of the lower wire in the tube, which communicated with the silver, and the opposite point of the upper wire became tarnished, first deep orange and then black. On reversing the tube, the gas came from the other point, which was now lowest, while the upper in its turn became tarnished and black. . . . The product of gas, during the whole two hours and a half, was two-thirtieths of a cubic inch. It was then mixed with an equal quantity of common air, and exploded by the application of a lighted waxed thread." "We had been led by our reasoning on the first appearance of hydrogen to expect a decomposition of the water; but it was with no little surprise that we found the hydrogen extricated at the contact with one wire, while the oxygen fixed itself in combination with the other wire at the distance of almost two inches. This new fact still remains to be explained, and seems to point at some general

law of the agency of electricity in chemical operations." Subsequently "Two wires of platina . . . were inserted into a short tube of a quarter of an inch inside diameter. When placed in the circuit, the silver side gave a plentiful stream of fine bubbles, and the zinc side also a stream less plentiful. . . . It was natural to conjecture, that the larger stream from the silver side was hydrogen, and the smaller oxygen. . . ." Carlisle and Nicholson had by now each made a pile for himself; these were combined, and the two gases were collected in quantity. "The two platina wires were made to protrude out of two separate tubes . . ." which "were plunged in a shallow glass vessel of water, in which two small inverted vessels, quite full of water, were so disposed, that the platina of one tube was beneath one vessel, and the platina of the other tube was beneath the other. . . . A cloud of gas arose from each wire, but most from the silver, or minus side. Bubbles were extricated from all parts of the water, and adhered to the whole internal surface of the vessels. The process was continued for thirteen hours, after which the wires were disengaged, and the gases decanted into separate bottles. On measuring the quantities, which was done by weighing the bottles, it was found that the quantities of water displaced by the gases were respectively, 72 grains by the gas from the zinc side, and 142 grains by the gas from the silver side. . . . These are nearly the proportions in bulk, of what are stated to be the component parts of water." (See William Nicholson's account in *Nicholson's Journal*, July 1800, pp. 179–91.)

The electrolysis of water by Carlisle and Nicholson was the first complete and definite instance of the decomposition of a chemical compound effected by means of the galvanic current. Von Humboldt and others had, indeed, already pointed out certain phenomena which obviously depended upon chemical actions of the current. It had been suspected, even before the invention of the Voltaic pile, that perhaps chemical changes might be, not the consequences, but the causes, of the generation of electricity. Still the two English investigators must have the credit of having demonstrated the galvanic decomposition of water for the first time by a carefully contrived and illuminating experiment. The next step was to apply the new instrument to substances of unknown chemical constitution, and this step was taken with brilliant results a few years later by Humphry Davy.

RITTER AND WOLLASTON

In Germany J. W. Ritter was among the first who concerned themselves with the problems of galvanic electricity. Like Volta he recognized that galvanic phenomena may occur without the presence

of an animal body; and he further proved the identity of galvanic and ordinary electrical phenomena—a topic which continued for some time still to be debated. Ritter showed that the two poles of a pile exert an attraction upon each other, by attaching a wire to each pole, and a strip of gold-leaf to the end of each of these wires. Upon bringing the wires near together, the gold leaves attracted each other until they finally made contact, and so closed the circuit (*Gilbert's Annalen*, VIII, p. 390). An experiment on somewhat complementary lines was performed by W. H. Wollaston, who decomposed a copper sulphate solution between two fine wire electrodes connected to the terminals of a frictional electrical machine (*Phil. Trans.*, 1801, p. 427).

F. MAGNETISM

During the greater part of the eighteenth century the phenomena of magnetism were explained as due to Cartesian vortices, which were supposed to circulate about magnetic bodies. In the "one-fluid" form of this theory, taught by Euler, such bodies were conceived as honeycombed by fine valve-like pores through which the magnetic fluid could pass in one direction only. It thus entered the magnet at one pole and left it at the other pole. In the latter part of the century, however, physicists tended to apply to magnetism the hypothesis of an all-pervading fluid, or of two such fluids, which had been found to work well in explaining the analogous phenomena of electricity. Thus a "one-fluid" theory of magnetism (as of electricity) was advocated in 1759 by Aepinus. He conceived the fluid as composed of particles which repelled one another and attracted those of ordinary matter. A body was magnetized when the bulk of the fluid which it contained was accumulated at one end of it. The difficulty of explaining the mutual repulsion of negative poles, on this hypothesis, naturally led to a "two-fluid" theory, such as was suggested soon afterwards by Anton Brugman, and was later embraced by Coulomb and Poisson. In the form which this hypothesis usually assumed, the ultimate particles of a magnetic substance, or certain of them, were conceived as miniature magnets, or as capable of becoming such under the action of a magnetic field. On such an hypothesis, which was justified by the experimental impossibility of obtaining isolated poles, magnetization simply consisted in orientating the irregularly distributed magnetic particles into chains.

The study of magnetic attraction and repulsion was facilitated during the eighteenth century by considerable advances in methods of manufacturing strong artificial magnets. Some of the best of these were the work of Gowan Knight. He proceeded by separately

magnetizing a number of steel strips by a special method of his own, and then binding them up into bundles or magazines which were armed with iron so as to form compound magnets having remarkable lifting-power (*Phil. Trans.*, 1744, p. 161, and 1747, p. 656). Further improvements in this direction were suggested by John Canton, but all such methods were superseded in the nineteenth century after the discovery of electro-magnetism.

A few fresh discoveries in magnetism were made during the period under review. It was found that other substances besides iron are influenced by a magnet, and that such influence is not in all cases one of attraction. Thus the new elements cobalt and nickel were shown by their respective discoverers, Brandt (1735) and Cronstedt (1751), to be slightly magnetic, and the list was later extended by Coulomb. On the other hand, Sebald Brugman (son of Anton), in 1778, showed that bismuth and antimony repelled the poles of a magnetic needle—the earliest known instance of *diamagnetism*. The magnetization of iron by lightning also received considerable attention during this period.

COULOMB

The greatest achievement of the eighteenth century in magnetism, however, was the determination by Coulomb of the law according to which the force of a magnetic pole varies with distance. Newton in his *Principia* describes some crude attempts of his own to ascertain this law, and during the following century a series of inconclusive investigations on the problem were carried out. The usual procedure was to mount a magnetic needle at the centre of a graduated quadrant, and to read off the deflection produced in the needle by a lodestone which was presented to it in a direction perpendicular to the magnetic meridian. The deflection of the needle varied according to the distance of the stone, and it could be tabulated against this distance. Experiments on these lines were carried out by Halley and were described by him in a paper read to the Royal Society on March 2, 1687 (*Register Book*, Vol. 9, p. 25). Similar observations were subsequently made by Hauksbee (*Phil. Trans.*, 1712, p. 506) and by Brook Taylor (*Phil. Trans.*, 1721, p. 204). But none of these arrived at any definite law of force. Musschenbroek used a balance to compare the attractions of two magnets at various distances apart; but his results were likewise inconclusive. From an examination of the results obtained by these investigators, however, Michell was led in 1750 to state that magnetic force probably obeys the inverse square law. Michell's statements occur in his *Treatise of Artificial Magnets* (Cambridge, 2nd ed., 1751). He holds

that "Each Pole attracts or repels exactly equally, at equal distances, in every direction. . . . The Magnetical Attraction and Repulsion are exactly equal to each other. . . . The Attraction and Repulsion of Magnets decrease, as the Squares of the distances from the respective Poles increase." Michell deduced this law "from some experiments I have made myself, and from those I have seen of others. . . . But I do not pretend to lay it down as certain, not having made experiments enough yet, to determine it with sufficient exactness" (*op. cit.*, pp. 17–19). The same law was soon afterwards formulated by J. T. Mayer, whose memoir, submitted to the *Kgl. Gesellsch.* of Göttingen, was apparently not published; but Fischer states that there is an account of the contents in Erxleben and Lichtenberg's *Anfangsgründe der Naturwissenschaft*, § 709. The Law was subsequently also deduced by Lambert, who had compared the curves obtained by plotting the field of a magnet, by means of an exploring compass, with the curves of force calculated on the assumption of an inverse square law (*Mém. de l'Acad. Roy. des Sciences de Berlin*, 1766).

In his classic determination of the law of magnetic force, Coulomb employed two independent methods which he described in a paper published in the *Mémoires de l'Académie Royale des Sciences* (Paris) in 1785 (Ostwald's *Klassiker*, No. 13).

In the first of these methods he used a short compass-needle free to oscillate about its mean position, and he vertically suspended a magnetized steel wire about twenty-five inches long in the same magnetic meridian as the needle, with one of its poles level with the latter. The period of oscillation of the needle for small amplitudes was noted, first under the action of the Earth's field alone, and then with the vertical magnet held at different small distances from it. Assuming the oscillation to be simple harmonic, the intensity of the field should vary inversely as the square of the period. By suitably combining the results, the effect of the Earth's field was eliminated, and the force due to the magnet was shown to vary inversely as the square of the distance from the attracting pole. In his actual experiments, Coulomb took various disturbing factors into account. Thus he found what distance should be regarded as the effective distance of the long magnet from the needle; where the pole of the long magnet should be considered to lie; how far the action of the other pole should be considered, etc.

In his second method Coulomb employed a torsion-balance somewhat similar to that which he used to determine the law of electrical repulsion. His instrument consisted essentially of a box containing a graduated circle and spanned by a cross-bar, through a central hole in which was passed a vertical tube. Inside this tube

was a brass wire attached at its upper end to a torsion-micrometer capable of turning the wire through any angle, which could be read off the graduated head of the micrometer. At its lower end the wire carried a stirrup in which a bar-magnet was placed ; and the apparatus was initially arranged so that the wire was free from torsion, the reading on the micrometer was zero, and the magnet in the stirrup lay in the magnetic meridian pointing to 0° on the lower circle. The wire was now twisted so as to turn the magnet out of the meridian through a measured angle. The amount of torsion required to do
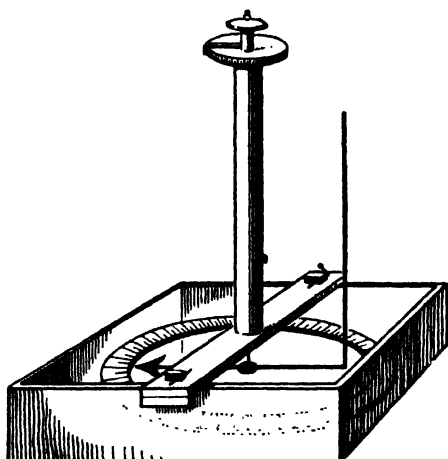
Illustr. 127.—Coulomb's Magnetic Torsion-Balance

this gave a measure of the strength of the Earth's field. The magnet was then brought back into the meridian, and a long magnet was placed vertically in the meridian so as to repel the suspended magnet. The latter was then gradually brought back to its starting-point by twisting the wire, and the torsion exerted at each angular separation of the two poles was noted. Allowing for the torsion due to the Earth's field (known from the preliminary experiment) it was possible to ascertain how the repulsion between the two poles varied with their distance apart, and the inverse square law was thus confirmed.

A still more decisive experimental confirmation of the law was obtained by Gauss early in the nineteenth century.

MAGNETIC DECLINATION

Numerous observations of the magnetic declination, or variation of the compass, were made during the eighteenth century, and it was found necessary periodically to revise Halley's variation-chart

for 1700. This was done by William Mountaine and James Dodson. In 1757 they published sets of tables based upon thousands of observations taken during half a century, and showing the distribution of the variation for the years 1710, 1720, 1730, 1744, and 1756 (*Phil. Trans.*, Vol. L, p. 329). The earliest chart claiming to show the distribution of magnetic dip over a considerable portion of the Earth's surface by means of a series of curves drawn through sets of places where the inclination of the needle to the horizontal was the same, was published by Wilcke, in Sweden, in 1768 (*Kongl. Vetenskaps Academiens Handlingar*, Vol. XXIX). Two still earlier isoclinic charts were, indeed, constructed by William Whiston, mainly on the basis of his own measurements of dip, and were published in his book *The Longitude and Latitude found by the Inclinatory or Dipping Needle*, etc. (London, 1721). But Whiston's charts covered only the southeastern part of England, and the midland and south-eastern parts, respectively; and the isoclinic curves shown upon them appear as parallel and equally spaced straight lines, suggesting a conventional distribution. Wilcke's chart, on the other hand, covers Europe, the Atlantic Ocean, South America, Africa, the Indian Ocean, and part of the Pacific, but not Asia nor North America. The observations from which the chart was constructed are all recorded upon it in the appropriate positions, though Wilcke had no illusions about the uncertainty attaching alike to these estimates of dip, and to the precise longitudes and latitudes of the places at which they were made. (See Hellmann's *Neudrucke*, No. 4, where Whiston's and Wilcke's charts are reproduced with notes.)

The existence of diurnal fluctuations in the variation of the compass was established by George Graham as a result of a series of observations made in 1722–3 (*Phil. Trans.*, 1724, p. 96). This effect was later confirmed by Celsius, who also drew attention to the close connection between disturbances of the magnetic needle and auroral displays—a correlation later studied by Wargentin and Dalton. In 1756 Canton took up the question both of the diurnal and of the irregular fluctuations in the variation of the compass, and he made a series of about four thousand observations of this quantity. He attributed the diurnal fluctuations to the unequal heating of the Earth's surface by the Sun, and the irregular disturbances he ascribed to subterranean heating which, he supposed, gave rise also to the accompanying auroral displays somewhat as heating a tourmaline generates electricity in it (*Phil. Trans.*, 1759, p. 398). He noticed that the amplitude of the diurnal fluctuation in summer was nearly double what it was in winter. Annual fluctuations in the variation of the compass were also observed by Count Cassini about 1780.

Attempts to compare the intensities of the Earth's magnetic field at various times and places began to be made towards the close of the eighteenth century. The method employed was to compare the frequencies of oscillation of a given needle under the various conditions. Pioneers in this field were the Swede, F. Mallet (1769, *Novi commentarii Academiae Scientiarum Petropolitanae*), the French physicist Borda (1776), and the explorer Humboldt, about the end of the century. The absolute measurement of magnetic intensity was first achieved by Gauss, in the nineteenth century.

Humboldt constructed a chart showing the zones of the Earth's surface over which the intensity of the terrestrial magnetic field was roughly the same throughout. This was based upon measurements made in the course of Humboldt's American journey (1799-1803), and published in a paper *Sur les variations du magnétisme terrestre à différentes latitudes*, par MM. Humboldt et Biot, read to the *Institut National, 26 Frimaire, an* 13. The data were obtained by allowing a dipping-needle to oscillate in the magnetic meridian, and observing the number of oscillations occurring in ten minutes. Humboldt regarded his discovery that the intensity decreases from the Pole towards the Equator as perhaps the most important result of his American voyage. The first detailed isodynamic charts were published by C. Hansteen in 1825 and 1826. (Humboldt's chart is reproduced with notes in Hellmann's *Neudrucke*, No. 4.)

Apart from such speculations as Canton's explanation of the diurnal oscillations of the needle (which was revived for a time in the nineteenth century), little contribution was made during the period here reviewed to the *theory* of terrestrial magnetism. Halley's hypothesis of four magnetic poles was abandoned by Euler, who, following Descartes, regarded slow changes in the variation as due to the generation and decay of iron in the Earth's interior, and therefore as unpredictable.

(See P. F. Mottelay, *Bibliographical History of Electricity and Magnetism*, 1922; E. Hoppe, *Geschichte der Elektrizität*, Leipzig, 1884; and the general books on Physics on p. 172.)

METEOROLOGY

A. Meteorological Literature

Representative works illustrating the development of the literary treatment of meteorology during the eighteenth century are those of Christian Wolff, Michael Christoph Hanov, Louis Cotte, and John Dalton. These writers completed the task, begun in the seventeenth century, of emancipating the subject from the age-long influence of Aristotle's *Meteorologica*, and of establishing, as a branch of applied physics based upon observation, what had long been a mere appanage of astrology. It was, however, comparatively late in the eighteenth century before expositions of the subject began to be markedly influenced by the results of actual instrumental observations. Before meteorology was recognized as an independent branch of science whose laws must be ascertained by systematic observation of atmospheric phenomena, it passed through the stage of being treated in a jejune and formal manner as a part of elementary pneumatics.

WOLFF

The dry and formal treatment of meteorology is exemplified in an elementary treatise by Christian Wolff (1679–1754), the philosopher. This work bears the title *Aerometriae Elementa, in quibus aliquot Aeris vires ac proprietates juxta methodum Geometrarum demonstrantur*, Lipsiae, 1709. It is strictly mathematical in form, proceeding by way of *definitions* (e.g., that "aerometry is the science of measuring the air"), explanatory *scholia* (explaining, for instance, what is meant by *science*, by *measurement*, and by *air*), and *axioms* (e.g., that heavy bodies press perpendicularly downwards on others placed below them), to *theorems* (e.g., that the pressure of air acts in all directions, that the *elater* or spring of the air equals the weight of the superincumbent column of the atmosphere, etc.), and *problems* (e.g., to construct an air-pump). There is very little reference throughout to actual observations, apart from a few basic experiments, such as Galilei's observations on the weight of air, and on the properties of the water-pump; Torricelli's experiment with the mercury barometer; the thermal expansion of water and of air; the establishment of Boyle's Law, etc. In one of the few sections dealing with purely meteorological topics, Wolff regards winds as caused almost exclusively by sudden local expansions or contractions of the atmo-

spher   the heat of the Sun being the]   ipal factor in causing such displacements of equilibrium. The methods mentioned for measuring the height to which the atmosphere extends are those depending on estimates, either of the duration of twilight (a method known since the Middle Ages), or of the barometric pressure at ground level taken in conjunction with the law connecting the density of the air with height above the surface of the Earth (the method of Mariotte and Halley). Wolff does little more than define the chief meteorological instruments, though he gives rather fuller details, and illustrations, where the hygrometer and anemometer are concerned. For the measurement of humidity he suggests making a hygrometer of a long string of hemp fastened at one end to a wall, passing horizontally along the wall over a pulley-wheel fixed thereto, and supporting at its free end a weight which keeps the string taut. Expansions and contractions of the string, due to alterations in the humidity of the air, turn the pulley-wheel one way or the other through angles which are shown by the movements of a pointer attached to the wheel and moving over a graduated dial. Wolff also describes an interesting anemometer, in which the wind drives a small windmill whose axle turns a cog-wheel by means of an endless screw. To this wheel is attached a radial arm, having a weight fixed at the farther end. When the instrument is not in use, this arm hangs vertically downward. As the force of the wind turns the cog-wheel and the attached arm, the weight is raised; but it exerts a steadily increasing resistance to being raised, and it brings the whole mechanism to a standstill when its couple upon the wheel equals that exerted by the force of the wind acting through the screw. The angle through which the cog-wheel is turned before thus coming to rest measures the force of the wind; and it is shown by a pointer which is attached to the wheel and moves over a dial on the case of the instrument. An illustrated description of the air-pump is given; and it is proved that, when the receiver has been partially exhausted by n strokes of the pump, the residual quantity of air in the receiver is to the original quantity, as the nth power of the capacity of the receiver is to the nth power of the united capacities of the receiver and cylinder.

HANOV

The exposition of Christian Wolff's system of natural philosophy was continued after his death by Michael Christoph Hanov, in his *Philosophia Naturalis sive Physica Dogmatica*, Halae Magd., 1762–8. In this work, however, meteorology enjoys somewhat greater autonomy, the second of Hanov's four volumes being entirely devoted to "aerology and hydrology."

The properties of pure air are first studied, in abstraction from those of the vapours with which it is commonly charged. Besides a full treatment of pneumatics, and of pneumatical instruments, there is a separate account of winds. Their cause is given as a disturbance of the equilibrium of the atmosphere, due, for example, to local exhaustion, absorption, or thermal dilatation, resulting in an inrush of the surrounding air. The functions of winds are, to carry away bad air, to temper heat and cold, to distribute clouds and the vapour upon which the fertility of the soil depends, to promote evaporation from wet ground, to assist navigation, and to drive mills. As for the measurement of wind, Hanov describes his own attempts to compare wind-strengths by exposing a series of flags of various lengths, and noting which of them the wind was just strong enough to blow out horizontally. Alternatively, he used a single flag loaded with various weights; or he measured the deflection of a horse-hair to which a small leaden weight was attached. In this way he managed to distinguish 8 degrees of wind-strength. In contrast to pure air, the Earth's atmosphere is a "promptuary" of vapours and exhalations, which it absorbs like a sponge, and whose proper distribution ensures the fertility of the soil. The absorptive activities of the atmosphere seem to have been conceived on a magnetic analogy, the particles of air being regarded as endowed with attractive poles enabling them to link up to form retentive chains. The atmosphere is divided into three regions, which are, in order of distance from the centre of the Earth, (1) a layer about $4\frac{1}{2}$ miles thick, extending from the bottoms of the deepest pits to the summits of the highest mountains; (2) an intermediate layer about $5\frac{1}{2}$ miles thick, which is the region whence the Sun's light is reflected to form twilight; and (3) an upper layer, perhaps 50 miles thick, which is the scene of auroras, and is reached only by the most subtile exhalations. Atmospheric phenomena, or *meteors*, are divided into two classes, *aethereal* and *aqueous*. Aethereal meteors are those which give light and heat (whose vehicle is supposed to be the aether): they include (as generally arising from the combination of exhalations) falling stars, *ignes fatui*, auroras, haloes, parhelia, colorations of the sky; also rainbows (explained according to Newton), and thunderstorms (regarded as explosions of nitrous sulphur mixed with watery vapours). Watery meteors may be liquid or frozen; the liquid sort may be vaporous (clouds, mists, etc.), or condensed (rain, dew, etc.); the frozen may be hoar-frost (vapours frozen on to bodies), or snow (vapours frozen in the air). In the section on hydrology, lakes, rivers, mineral springs, etc., are discussed on traditional lines; the sea is declared to have been *created* salt in the beginning; and the tides are explained as due to a disturbance

of the equilibrium of the aether caused by the rotation of the Earth.

COTTE

The *Traité de Météorologie* of Père Louis Cotte (1740–1815), *curé* of Montmorency, near Paris, and friend of Rousseau, was published at Paris in 1774, under the auspices of the *Académie des Sciences*. Cotte was a correspondent of the Academy, and he drew extensively upon its memoirs in the composition of his treatise, which was the first text-book to be based upon observations. The bulk of the work is divided into five Books, but there is first an introductory section on the history of meteorological observation in France, in which Cotte traces regular observations back at least to the foundation of the Academy of Sciences in 1666. Mariotte and Picard were pioneers in the subject, and Morin kept, for upwards of thirty years, an exact meteorological journal. From 1688 onwards the Academy had a regular record constantly kept by one of its members. Réaumur, after his improvement of the thermometer, organized thermometric observations all over the world between 1733 and 1740. Mairan catalogued occurrences of the aurora borealis for some years. And many foreign correspondents sent in reports which were published from time to time in the *Recueil des Mémoires des Savans Etrangers*. Besides such purely meteorological records, there were others, such as those kept from 1741 onwards by Duhamel on the relation of weather to botanical phenomena, and those made between 1746 and 1754 by Malouin with a view to ascertaining how different kinds of weather affected the course of certain diseases. Of all these records, and of many others in print or manuscript, Cotte was able to make use; and he himself was a keen and experienced observer. He regarded the collection of observations as of service to agriculture and medicine, and an indispensable preliminary to the construction of the scientific meteorology which he hoped would one day arise. Atmospheric phenomena, he admitted, seemed disorderly enough, but were perhaps less so than they seemed; and careful, long-continued observation might bring regularities to light.

Book I of Cotte's treatise deals with the atmosphere (its composition, height, and pressure, its vicissitudes of heat and cold, and its electrical properties), and with the various kinds of atmospheric phenomena (*météores*). These are classified as (i) *aerial* (winds and waterspouts); (ii) *aqueous* (dew, fog, rain, etc.); (iii) *fiery* (thunder and lightning, St. Elmo's Fire, will-of-the-wisps, earthquakes, etc.); and (iv) *luminous* (rainbows, mock-suns, auroras, etc.). The principal explanations of these phenomena then current are described. Book II deals with meteorological instruments, reviewing their history, giving

instructions for the proper manufacture of the chief types, and mentioning the characteristic drawbacks of each. Some twenty-five different kinds of thermometers are described in more or less detail; and other sections deal with the barometer (with its application to measuring heights), hygrometer (which Cotte thought still too faulty for scientific use), anemometer, rain-gauge, compass, and electrometer. There are a number of illustrative plates. Book III contains fifteen tables of meteorological, botanical, and demographic interest, many of them based upon records accumulated by the Academy. These tables respectively show: (i) the greatest and least degrees of heat (on Réaumur's scale) observed at Paris in each year from 1699 to 1770; (ii) some temperatures of the surface of the sea taken at different times of the day and year, and in water of various depths, with the temperature at sea-bottom for comparison; (iii) the greatest and least barometric readings at Paris for the years 1699–1770; (iv) the prevailing winds and prevailing weather, for the years 1748–70; (v) the annual rainfall in Paris, 1689–1754; (vi) a comparison of annual rainfall in Paris and in other towns in West Europe; (vii) the variation of the compass in Paris from 1580 to 1770; (viii) auroral displays set out month by month from 1716 to 1734, with some earlier data going back to A.D. 500; (ix) a conspectus showing the averages of the preceding tables; (x) the dates at which various fruits and crops flowered or came to maturity during 1741-70; (xi) the dates at which the swallows came and went, the nightingale and cuckoo began to sing, and certain insects appeared, over the same period of years; (xii) the sum of the degrees of heat (formed by summing the daily mean degrees of heat) falling on the land in April–June, for each of the years 1748–70, which are classified as hot or cold, wet or dry; (xiii) the mean degrees of heat and cold for each day in the year; (xiv) the annual numbers of births, marriages, and burials (with distinction of sex) at Montmorency (Cotte's parish) from 1701 to 1770; (xv) the totals of these for each of the twelve months taken over the same years. In Book IV, which Cotte regarded as the essential part of the whole work, the results set out in these tables are discussed in detail, and conclusions are drawn from them. The Book is divided into three sections, Physico-Meteorological, Botanico-Meteorological, and Medico-Meteorological. The nature of the contents may be illustrated by a few of the points dealt with. Cotte shows that the average value of the highest temperatures over many years exceeds the freezing-point by about four times as much as the average of the lowest temperatures falls below the freezing-point. The greatest heat and the greatest cold of the year lag by about forty days behind the corresponding solstices; and similarly, the greatest heat and cold of the day occur about three hours after

noon and after midnight respectively. Comparison of temperature data from all over the world gave Cotte the impression that the *degree* of summer heat is everywhere about the same, whether on the Equator or on the Arctic Circle, the heat, however, being more uniformly maintained in torrid latitudes, where the inhabitants are subject to less violent changes of temperature. The *sensation* of heat, moreover, is more insupportable in equatorial regions, because of the heat accumulated in bodies exposed to the Sun. Cotte distrusted the barometer as an instrument for foretelling the weather, though he gave rules which he thought as trustworthy as such rules could be expected to be. He thought that corrected barometric readings at widely separated places showed considerable agreement; and he was under the impression that, at least in the torrid zone, the height of the barometer showed some connection with the phases of the Moon. He regarded winds as the chief agents in producing changes of weather. The results obtained with the other instruments are also discussed, and the section closes with some select observations sent in from certain stations in France and abroad, e.g., Mexico, Quebec, Vilna, the Cape, etc. In the botanical section, Cotte seeks to establish relations between meteorological conditions and the growth of the fruits of the soil, while admitting that the determining factors are so numerous that he cannot guarantee the absolute soundness of his conclusions. After a chapter dealing with the motion of the sap in plants, and another treating of the several sorts of soil, he considers in turn the influence of different kinds of weather upon the growth of wheat, rye, oats, barley, hay, and fodder in general, fruit-trees and vines. This enquiry is undertaken in the hope that it will enable farmers to protect their crops against injurious conditions, and give naturalists the clue to the causes and possible remedies of common plant diseases. A multitude of small observations are set down, and generalizations are made where possible, but nothing of much scientific value emerges, though the new emphasis on observation is significant. Next are considered the various birds of passage (whose movements are ascribed rather to the search for food than to changes of temperature); insects of agricultural importance, and, lastly, circumstances determining the heights of rivers at various seasons of the year. The factors conditioning health and disease, studied in the third section (which is almost entirely based on Malouin's work), are the pressure, humidity, temperature, and composition of the atmosphere, winds, food and water, climate, and manner of life. The attempt to trace certain diseases to certain factors of this kind is naturally of little value. The Book concludes with observations on the vital statistics of Montmorency, already tabulated in Book III. Book V contains instructions for carrying out meteorological observa-

tions, based on Cotte's own experience, with particular reference to the ideal qualities of the observer (who should preferably be a physician), the best site for the observatory, the choice of instruments, precautions in their use, and the best method of recording and summarizing the observations made. By way of example Cotte gives his own recorded observations for the year 1771, with a physical, botanical, medical, and demographic *résumé* for that year at Montmorency.

Cotte found it necessary to supplement his massive Treatise with two large volumes of *Mémoires sur la Météorologie*, published at Paris in 1788. In the meantime he had become Canon of Laon Cathedral, and a member of the *Mannheimer Gesellschaft*. General interest in meteorological science had been stimulated, not only by the formation of that fellowship, but by the meteorological activities of the *Société Royale de Médicine* of Paris (with which Cotte was closely associated), the establishment of the *Natuur- en Geneeskundige Correspondentie Societeit* at the Hague (with the editor of whose memoirs, J. H. van Swinden, Cotte was in friendly correspondence), and by the publication of De Luc's important work on instruments in 1772, which appeared too late to be of much use to Cotte in the composition of his earlier treatise. Cotte's memoirs deal with such problems as the best methods of drawing up observations, the causes of cold and heat in the atmosphere, the question of whether the Moon influences vegetation (which is answered affirmatively), the etiolation of plants caused by absence of light, the influence of atmospheric electricity on weather and vegetation, experiments on the rate of evaporation of water, the improvement of hygrometers, with a comparison of the performances of various types, and a mass of technical detail on the construction and correct use of various meteorological instruments derived from De Luc and other contemporary adepts. The memoirs conclude with extracts and summaries (occupying some 420 pages) of observations made at stations all over the world. Cotte was a prolific writer, and made many contributions on meteorology to the memoirs of learned societies with which he was in touch.

DALTON

The subordination of meteorological theory and speculation to the claims of systematic observation, characterizing the latter part of the eighteenth century, is again noticeable in the *Meteorological Observations and Essays* of John Dalton, the chemist. This book, which was published in 1793, consists of two sections, the first dealing with instruments and observations, and the second comprising eight Essays of a more speculative character.

In the former section, Dalton gives instructions for making the common meteorological instruments, and brief accounts of the principles on which they work. Of more significance are the tables summarizing observations of barometric height, temperature, humidity, rainfall, and direction and strength of wind, which were made regularly during the years 1788–92 by Dalton himself at Kendal, and by his friend Peter Crosthwaite at Keswick. Observations made in London during part of this period are quoted from the *Philosophical Transactions* for comparison. Such comparison shows the maxima and minima of the barometric readings to have occurred at the same, or very nearly the same, dates at all the three stations. Dalton made a special study of the seasonal fluctuations in the temperatures of wells, showing that these were very slight if the wells were deep. He used as a hygrometer six yards of whip-cord, which was fastened to a nail at one end, passed over a pulley-wheel, and stretched by a small weight whose rise and fall with changes in the humidity of the air was measured against an adjoining scale. Among other topics with which this portion of the book deals are Crosthwaite's observations on the heights of the clouds. He measured these, morning, noon, and evening, for five years, with the aid of a telescope, against landmarks on the side of Skiddaw whose respective heights above the level of Derwentwater had been previously ascertained. Crosthwaite's table shows for each of the twelve months of the year the number of occasions during the whole period on which the clouds were seen at heights of 0–100 yards, 100–200 yards . . . 900–1000 yards, 1000–1050 yards (the estimated height of Skiddaw), above the level of the lake, as well as the number of times when they rose above the mountain altogether. These observations by no means bore out the common supposition that the height of the clouds rose and fell with the barometer. Other chapters contain records of the occurrence of thunderstorms and hailstorms in the neighbourhood of Kendal, the distance of a thunderstorm being calculated in some cases from time-intervals observed between flashes and peals. The number of thunderstorms a month was found to be greatest in July. There are also records, for 1788–92, of the relative frequencies of the winds (from eight different points of the horizon) at Kendal and Keswick; of the dates of the first and last snow, and first hoarfrost, of the year; of aurorae boreales (account being taken of the peculiar character of each display, and of the age of the Moon when it occurred); and of the occurrence of the mysterious "bottom-winds" agitating Derwentwater in otherwise calm weather.

Of the Essays with which Dalton's book concludes, the first treats of the atmosphere, its constitution, temperature, and extent, with

some worked examples on the barometric determination of the heights of mountains. In the second Essay, dealing with winds, Dalton correctly refers the properties of trade winds to the natural circulation of air in the torrid zone, as modified by the fact that the Earth's surface moves eastward with different speeds in different latitudes. George Hadley had anticipated this explanation in 1735, and Dalton became aware of this just in time to acknowledge it in his preface. Dalton regarded the observed circulation of winds as evidence of the Earth's rotation, and as providentially adapted to promote the necessary mixture of the aerial fluids, and the intercourse of mankind. The third Essay discusses current speculations as to the cause of the variations in the barometric height. Dalton considers, only to reject, the suggestions that these are due to the collision of opposing winds, to condensation by the influx of cold air, to upward or downward blowing winds, to heating and cooling leading to local variations in the density of the air and hence in its average height and centrifugal tendency, etc. His own view is that the fluctuations of the barometer are due to variations in the density of the lower strata of the atmosphere, arising from the changes in their content of moisture caused by the influx of wet air into dry air, or of dry air into wet air. The temperate zones, where such influxes must be most common, accordingly show most barometric instability. On the subject of heat (the fourth Essay), Dalton states that he has "nothing new to offer"; and his Essay on evaporation (the sixth) is unimportant. The fifth Essay is an account of some calculations of Richard Kirwan's claiming to give the mean annual temperature for every 5° of latitude (under ideal conditions), and to show how the mean temperature actually varies with altitude, and with distance from the coast. The seventh Essay is concerned with the relation between the barometric height and the chances of rain. From his recorded observations, Dalton was led to the following conclusions: "1st. The higher the barometer is above its mean annual state, the less rain there is. 2nd. The farther it is below its mean annual state, the more rain there is, till it comes to a certain point, after which the rain seems to decrease again." Dalton's eighth, and last, Essay contains some shrewd observations on the aurora borealis, and some rather daring speculations as to its nature. Dalton was in the habit of measuring the bearing and altitude of the vertex of the auroral arch with a theodolite; and from the results of some such measurements made simultaneously at Kendal and at Keswick, he estimated the site of the phenomenon as being about 150 miles above the surface of the Earth. He noticed that the auroral arches appeared symmetrically disposed with regard to the magnetic meridian, and that the magnetic needle was disturbed by the

presence of an aurora. Writing of the display of October 13, 1792, Dalton says: "When the theodolite was adjusted without doors, and the needle at rest, it was next to impossible not to notice the exactitude with which the needle pointed to the middle of the northern concentric arches: soon after, the grand dome being formed, it was divided so evidently into two similar parts, by the plane of the magnetic meridian, that the circumstances seemed extremely improbable to be fortuitous . . . the luminous beams at that time were all parallel to the dipping-needle . . . the inference . . . was unavoidable, that the beams were guided, not by gravity, but by the Earth's magnetism, and the disturbance of the needle that had been heretofore observed during the time of an aurora, seemed to put the conclusion past doubt." Here, again, Dalton found (as he acknowledges in his preface) that his ideas had been anticipated, this time by Halley. Dalton proceeds to describe the phenomena of the aurora in some detail, after giving the propositions on perspective which are necessary for their proper interpretation. As for their cause, "I consider it almost beyond doubt that the light of the aurora borealis, as well as that of falling stars and the larger meteors, is electric light solely, and that there is nothing of combustion in any of these phenomena." Dalton conceived the beams of the aurora, along which the electricity ran, as composed of an elastic fluid, partaking of the properties of iron, since nothing else was supposed to exhibit magnetism.

EPHEMERAL LITERATURE

Side by side with serious text-books and observational records of meteorology, much ephemeral literature has survived in the form of pamphlets describing exceptional atmospheric conditions, especially when accompanied by calamities or prodigies, such as storms, floods, "rains" of blood, frogs, etc. Such pamphlets (which are sometimes in rhyme) are often of interest as preserving descriptions of weather vagaries of which there is no other record. Typical of these booklets are those relating to the "Frost Fairs" held on the frozen Thames, one of which dates from 1740. Much of this occasional literature, from the time of the Reformation onwards, was written from a theological standpoint, and consists largely of printed sermons called forth by recent marvels and disasters, which, associated with appropriate texts, were expounded as signs of God's power, or as summonses to repentance. Thus a sermon has survived, preached by Dr. Blackall at St. Paul's on a fast-day (January 19, 1704) "Upon the Occasion of the Late Dreadful Storm and Tempest," the text (Luke xiii. 4, 5) relating to the fall of the Tower of Siloam. Such sermons were often supplemented by eye-witnesses' accounts of the events which

they commemorated; and references are made to similar local events in the past. Prayer-books for use during storms have also survived, some of which purport to explain the origin of these occurrences. This tendency to treat natural phenomena from a theological standpoint was confirmed in the eighteenth century by William Derham's books *Physico-Theology* and *Astro-Theology*, which, translated into several European languages, gave rise to a crop of books with such titles as *Bronto-Theology, Chiono-Theology, Hydro-Theology, Pyro-Theology*, etc.

(See G. Hellmann: *Beiträge zur Geschichte der Meteorologie*, Berlin, 1914, etc.)

B. CONCERTED METEOROLOGICAL OBSERVATION

Already in the seventeenth century, attempts had been made from time to time to compare meteorological observations taken simultaneously at a number of different stations. In the course of the eighteenth century concerted observations of this sort were made on an increasingly ambitious scale, by international networks of stations whose activities continued for years. The value of this work was enhanced, not only by improvements in the design and construction of meteorological instruments, but by a growing insistence upon the importance of using standardized instruments, and of following a uniform procedure in making observations, at the various stations, so as to ensure that the results sent in to headquarters should be readily comparable.

A start was made in Germany in the collection and systematic publication of meteorological data from an extended area early in the eighteenth century. A Breslau doctor, Johann Kanold, induced a number of weather-observers in Germany, and in some places abroad, including London, to send him their recorded observations. He published these for about ten years, beginning with the year 1717, in a quarterly journal which was generally known as the *Breslauer Sammlung*. Before this arrangement lapsed, another important step in the development of international meteorological organizations had been taken by James Jurin, Secretary of the Royal Society.

In 1723 Jurin appealed to all who were disposed and equipped for the work, to submit annually to the Society the records of their daily observations of the weather and of the readings of their instruments; and he drew up careful instructions for their guidance (*Phil. Trans.*, Vol. XXXII, p. 422). Jurin's wish was that observers participating in the scheme should, at least once a day, record the readings of their barometer and thermometer, the direction of the wind (with some numerical estimate of its strength), the quantity

of rain or snow-water collected since the last observation, and the appearance of the sky. Hygroscopic and magnetic observations would also be welcomed. Further, when any severe storm occurred, they were to note the time, and read the barometer, at its commencement, height, abatement, and end. As to instruments, Jurin recommended the use of a common barometer, consisting of a tube one-quarter or one-third of an inch in bore (narrower tubes depressed the mercury below its proper level), dipping into a trough whose diameter should be eight or ten times that of the tube, so that the level of the free mercury surface should be practically constant. Those preferring portable barometers were advised to obtain them from Francis Hauksbee in Crane Court, London, where thermometers of standard design and great precision could also be procured; if other types of thermometers were used, particulars as to their provenance and graduation were to be furnished. The thermometer should be located in a north room where a fire was seldom or never lighted. The rain-gauge, according to Jurin, should consist of a funnel two or three feet in diameter, emptying itself through a long stem into a graduated cylindrical measuring-vessel, kept as air-tight as possible so as to reduce losses by evaporation. This instrument was to be set up in a completely unsheltered position. The strength of the wind was to be estimated on a scale of four degrees ranging from 0 (a perfect calm) through 1 (the lightest breeze), 2, and 3, to 4 (the most violent wind)—a system which lasted on to the middle of the nineteenth century. The observations were to be recorded in a journal having six parallel columns, entries in which should respectively show (i) the date and hour of observation; (ii) the height of the barometer; (iii) the temperature; (iv) the direction and strength of the wind; (v) a concise description of the weather; and (vi) the aqueous precipitation (in inches and tenths of an inch). The averages of (ii) and (iii), and the total of (vi) were to be cast up for each month, and for the year as a whole; and the observers were to send up copies of their journals each year to the Secretaries of the Royal Society for comparison with one another, and with the Society's own weather-book. The results of such collation were to be published year by year in the *Philosophical Transactions*. From 1724 onwards, for a time, observers' journals were sent in, not only from Britain, but from many parts of Europe, from India, and from North America. They were discussed by Derham (*Phil. Trans.*, 1732, p. 261; 1733, p. 101; 1734, pp. 332, 405, 458), and subsequently by Hadley (*ibid.*, 1738, p. 154, and 1742, p. 243). Both Derham and Hadley were struck by the way in which variations in the barometric height agreed as between stations distributed over considerable areas (e.g., between London and Southwick, some

fifty miles away); though such variations would sometimes occur a little sooner or later in one place than in another. Despite Jurin's admonition, the value of the observations sent in was seriously reduced through observers not specifying the exact nature of their instruments, the situations and altitudes of their observatories, etc.

A somewhat similar scheme to Jurin's for comparing concerted observations was later organized by the *Société Royale de Médicine* of Paris. The purpose of the Society's enquiries, however, was to ascertain such correlation as might exist between meteorological conditions and the incidence of disease in the region covered. The observing stations were mostly in France, though a few other countries joined in. The results were collected and digested by Louis Cotte, priest and meteorological amateur, who published his reports in the Society's *Histoire* (1776–86), where the results were tabulated, and general conclusions were drawn from them. Cotte's important books on meteorology have already been described above.

During the greater part of the eighteenth century, the unsettled political conditions in Germany were unfavourable to schemes for co-ordinated meteorological activities in that country. But the latter part of the century saw established there the most successful of all the meteorological organizations of the period here considered. This was the *Societas Meteorologica Palatina* (or *Die Mannheimer meteorologische Gesellschaft*) which was founded in 1780 by the Elector Karl Theodor of Bavaria, with its headquarters in the Elector's castle at Mannheim, and with J. J. Hemmer, a distinguished meteorologist, as its first Director. Fifty-seven suitable institutions were chosen as observing stations. These extended from Siberia to North America, and southward to the Mediterranean, though England took no part. The stations were equipped, free of charge, with uniform sets of accurate instruments, together with exhaustive instructions for their use. The results obtained with them were entered on special forms provided, and sent in to Mannheim, where they were digested and published *in extenso*. The instruments supplied comprised a barometer, sun and shade thermometers, a quill-hygrometer, a rain-gauge, a wind-vane, an electrometer, and for some stations a magnetic needle. The amount of cloud and the strength of the wind were to be estimated numerically on conventional scales. Observations were to be made three times a day at definite hours: 7.0 a.m., 2.0 and 9.0 p.m. Symbols were to be used wherever possible in filling up the forms. Ever since the time when journals of meteorological observations began to be kept, it had been a common practice to use *abbreviations*, such as initial letters, for frequently recurring words. Later, arbitrary *symbols* began to be used to represent different sorts of weather, each observer choosing his own system. Such symbols began to

appear in print early in the eighteenth century. For example Van Musschenbroek, in his printed record of observations made at Utrecht in 1728, used such symbols as the following:

| (rain) | (snow) | (hail) | (lightning) | (cloudy sky) |
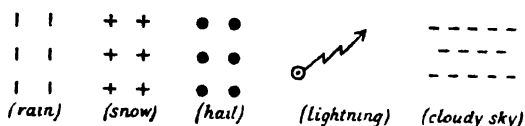
(*Physicae experimentales et geometricae*, Lugd. Batav., 1729). J. H. Lambert employed a different system in which astronomical symbols (O, ☾, ♀, etc.) were given meteorological meanings (*Acta Helvetica*, III, 1758). Systems of great complexity were proposed towards the close of the century, but they never became established. The more practicable system introduced by Hemmer, for the *Mannheimer Gesellschaft*, consisted partly of letters and partly of symbols. It owed something both to Musschenbroek and to Lambert, and traces of it still survive. The Mannheim Society's achievements are permanently embodied in its *Ephemerides*, which contain a vast mass of material that has been of great use in subsequent climatological researches. But Hemmer's death in 1790, and the political confusion following on the French Revolution, brought about the gradual collapse of the Society. Its last volume (for 1792) appeared in 1795, and no comparable organization arose to take its place until well into the nineteenth century. (See G. Hellmann: *Beiträge zur Geschichte der Meteorologie*, Berlin, 1914, etc., and *Repertorium der deutschen Meteorologie*, Leipzig, 1883.)

About twenty years after Jurin's invitation to weather-observers to send their records to the Royal Society, Roger Pickering submitted to the Society *A Scheme of a Diary of the Weather, together with Draughts and Descriptions of Machines subservient thereunto* (*Phil. Trans.*, Vol. XLIII, No. 473, p. 1). On each page of the diary seven vertical and nine horizontal lines were to be drawn. The vertical lines were to mark off the days of the week, each week occupying one page. Of the horizontal lines, the first was to show the days, and the second the hours of observation; on the third line was to be written for each day the corresponding barometric pressure; on the fourth, the temperature; on the fifth, the humidity; on the sixth, the direction of the wind; on the seventh, the force of the wind; on the eighth, a summary description of the weather; and on the ninth, the quantity of rain, etc., fallen since the last observation. Between the last line and the foot of the page, space was to be left for a bill of mortality. After the entries for each month, a page was to be left for a summary of the whole. The instruments recommended by Pickering were:

a common barometer consisting of a tube and cistern, and with a micrometer for reading the height of the column; a mercury thermometer with alternative scales; a hygrometer of the sponge and counterpoise type; a pendulum anemometer on the lines of that described by Hooke; and a rain-gauge consisting of a funnel and a graduated glass tube (Illustr. 128). Pickering's schemes, however, seem to have evoked very little enthusiasm, and successful meteorological societies were not established in England until the nineteenth century.

C. De Luc's Thermo-Barometric Study of the Atmosphere

About the middle of the eighteenth century, important progress was made in the design and construction of the barometer and the thermometer, and in the investigation of certain problems of the atmosphere, by Jean André De Luc (1727–1817), a Genevan geologist and physicist, who spent the latter part of his life in England. De Luc's most valuable contributions to physics are described in the two volumes of his *Recherches sur les Modifications de l'Atmosphère* (Geneva, 1772). In this book De Luc recorded and analysed the results of a long course of rigorous experimentation, and brought them into relation with the experiments and theories of his contemporaries. He did not work according to any predetermined plan, but was led on by the succession of problems arising out of his early physical experiments and Alpine travels with his brother. His exposition is therefore not very methodical, especially as he kept inserting fresh material while his book was actually in the press. He had no intention of publishing his researches at all, until urged to do so by La Condamine, at whose request he had communicated to the French Academy of Sciences the results of some early attempts to measure heights barometrically in the Alps.

The first of the five Parts, into which De Luc's work is divided, treats of the barometer. The history of the invention and development of this instrument is traced down to 1749, when De Luc started working upon it; fourteen different types of barometer are described, and several problems arising out of its properties are reviewed. Like Hauksbee, De Luc regards the phosphorescence occasionally observed above the mercury in an agitated barometer, as an effect of frictional electricity. With regard to the more important problem of what produces the incessant fluctuations in the height of the mercury column, De Luc reviews and criticizes the opinions of the principal seventeenth- and eighteenth-century writers on this subject, from Pascal to Musschenbroek. Of some note among the hypotheses mentioned are those of Leibniz and of Daniel Bernoulli. Leibniz
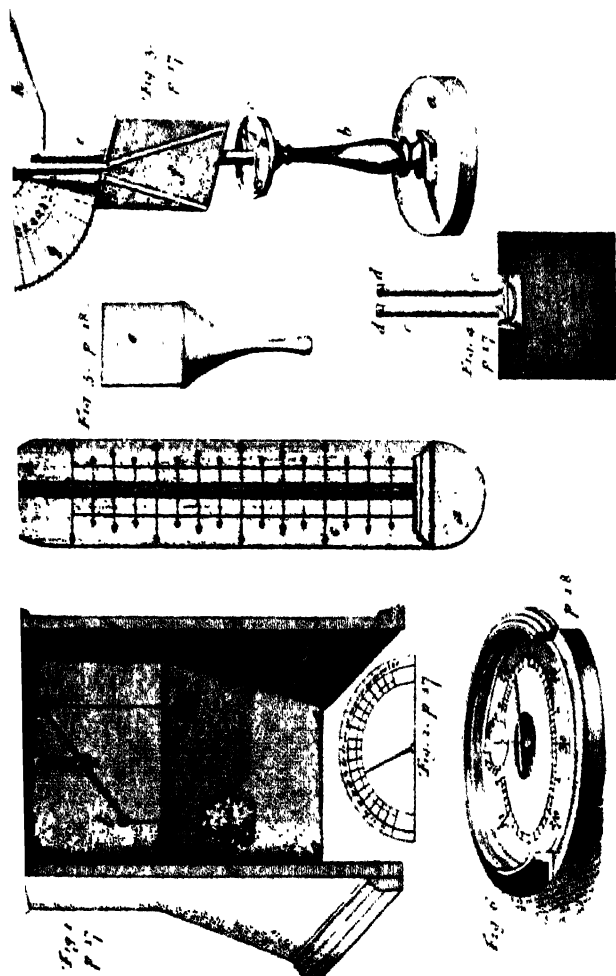
Pickering's Meteorological Instruments

Fig. 1—Sponge hygrometer with balance. Fig. 2—Graduated plate. Fig. 3—Anemoscope with circle marked with 32 points of the compass. Fig. 4—Anemoscope with the velum removed. Fig. 5—Rain-gauge and funnel. Fig. 6—Wild-oat hygrometer.

postulated that, when a body is supported by a fluid, it adds its own weight to that of the fluid, But if it ceases to be supported, and falls, the weight of the fluid becomes correspondingly less. Hence, when vapours in the upper atmosphere begin to liquefy, the barometric pressure is observed to fall, and rain can be predicted (Fontenelle in *Histoire de l'Académie des Sciences, année* 1711). Bernoulli thought that when air was heated in the cavities and pores of the Earth's crust it would rush forth and rise, so increasing the barometric pressure; on the other hand, when the internal heat decreased and the air contracted, the level of the atmosphere would sink, and the mercury would fall (*Hydrodynamica*, Section X). De Luc does not accept any of these hypotheses; he himself attributes the incessant fluctuations in the pressure of the atmosphere observed in most localities, to the varying amounts of specifically lighter vapours which are introduced into it. The less the proportion of vapour, the higher the barometer and the fairer the weather; the more vapour, the lower the barometer and the more likelihood of rain. De Luc expounds his hypothesis, and the evidence for it, in greater detail in Part IV, Chapter 9, of his book; and he also deals at great length with this type of problem in his *Idées sur la Météorologie* (London, 1786-87).

De Luc's chief interest in the barometer, however, centred in its application to the measurement of altitudes. The falling off in the atmospheric pressure with increase of altitude above the Earth's surface, had been known ever since the experiments of Périer and Pascal at the Puy-de-Dôme in 1648. Following the investigation by Halley, in 1686, of the law connecting atmospheric pressure with altitude, attempts were made to utilize the barometer for comparing the altitudes of different places, and for determining the heights of mountains. This procedure had the advantage over the older geodetic methods, of requiring neither the determination of a base-line, nor the application of conjectural corrections for refraction. De Luc reviews the observations and calculations carried out on this problem by the seventeenth-century pioneers and by their successors of the eighteenth century, Maraldi, the Scheuchzers, Jacques Cassini, Daniel Bernoulli, Horrebow, and Bouguer. Maraldi arrived at the rule that a rise of 61 feet from sea-level corresponded to a fall of one line in the barometric height; that a rise through the next 62 feet corresponded to a fall of a second line, and that subsequent rises of 63, 64 . . . feet would correspond to the same successive decrements of pressure (*Mém. de l'Acad. Roy. des Sciences*, 1703). This rule was adopted by Jacques Cassini; but the rules deduced by other observers differed widely from it. Thus, in 1709, Johann Jakob Scheuchzer found a difference of ten lines of mercury between the pressures at

the brink and at the foot of a cliff at Pfäffers in Switzerland, which he found by direct measurement with a cord to have a height of 714 feet (*Phil. Trans.*, 1728, p. 537, see also *ibid.*, p. 577). His brother formulated a rule connecting pressure with altitude (it was on the lines of Halley's formula, but involved different constants), while Johann Jakob's son, J. G. Scheuchzer, drew up a table mainly based upon this rule, and used it to estimate the heights of certain peaks to which trigonometrical methods had already been applied. As the agreement of the two sets of results was bad, Scheuchzer thought the trigonometrical methods must have been at fault. P. Horrebow, again, found that a rise of 75 feet above sea-level just produced a fall of one line in the mercury, and he drew up a table in which the successive elevations corresponding to successive decrements of pressure of this amount went up in harmonical progression. Lastly, Bouguer was led by numerous observations to the following rule: Take the difference (in lines) of the logarithms (to four figures) of the heights of the mercury at the foot and at the summit of a mountain; subtract one-thirtieth part, and you will have the height of the mountain expressed in fathoms. Bouguer and La Condamine had observed the barometric pressure in the Cordilleras, at elevated stations whose altitudes they had determined geodetically in the course of their expedition for measuring a meridian arc under the Equator (*Mém. de l'Acad. Roy. des Sciences*, 1753). Bouguer's rule connecting pressure with altitude seemed to hold good in the high Cordilleras, but, on Bouguer's own admission, not so well elsewhere. The serious divergence among the rules proposed by De Luc's predecessors is plain from the conspectus which he gives of the tables based upon them. In attempts to account for these discrepancies, recourse was occasionally had to *ad hoc* hypotheses about the physical properties of the atmosphere. Jacques Cassini suggested that volume might have to be taken as inversely proportional to the *square* of the pressure; and Daniel Bernoulli surmised that different layers of the atmosphere might be at different temperatures, their contributions to the pressure varying on that account. In all such cases of uncertainty, J. H. Lambert recommended a statistical procedure (*Beyträge zum Gebrauche der Mathematik und deren Anwendung*, Berlin, 1765, 1772). He took the problem of ascertaining heights barometrically as an illustrative example; the rule to be followed should represent the mean of all the available observations in which a certain degree of confidence could be placed, and the greatest departures of individual observations from this mean should indicate the degree of confidence to be placed in the rule. De Luc, however, regarded the available observations as too discrepant even for such treatment. He had begun to occupy himself with researches on the

barometer as early as 1749; but his zeal for the improvement of the instrument was intensified when, on returning with his brother from an Alpine expedition in 1754, he sought to calculate the altitudes of points which they had reached, from barometric observations made during the expedition. He soon found that the rules connecting altitude with barometric pressure, as given by different writers whose works he consulted, were in such disagreement that his observations were useless. These rules had been based upon scanty observations made with untrustworthy instruments, and they were influenced by preconceived ideas as to the constitution of the atmosphere. "I therefore resolved," writes De Luc, "to close the books and to consult Nature alone, following her step by step as far as she would lead me. True, I flattered myself that by the improvements that I had made in the barometer, I should easily complete a task which appeared to me to be a very useful one; it was this which made me enter on this course with confidence; but instead of finding a short and easy path, I plunged into a labyrinth whence I emerged only after much toil" (*Recherches*, Vol. I, p. 186).

The second Part of De Luc's book accordingly deals with the improvement in the construction and use of the barometer and the thermometer which was the foundation of all his subsequent advances in the quantitative study of the atmosphere. It was already generally recognized that the readings of barometers were influenced by factors having nothing to do with the pressure of the atmosphere. A number of barometers set up side by side might show different readings keeping no permanent relations to one another; and one such instrument might stand at a different height every time the tube was emptied of mercury and refilled. Some barometric observations on French mountains, recorded by Plantade and described by Cassini, brought to light the possible effects of the bore of the tube on the height of the mercury column. Again, the height of a barometer is affected by changes of temperature, producing corresponding changes in the density of the mercury. Amontons had drawn up a table of corrections for temperature to be applied to the barometer (*Mém. de l'Acad. Roy. des Sciences*, 1704). It was based upon the assumption that mercury expanded by $\frac{1}{115}$ of its volume in rising from the greatest cold to the greatest heat in Paris. The necessity for this correction, however, was ignored or denied by other physicists, and De Luc had once again to stress it. But he criticized the earlier methods of calculating the correction: they treated the temperature effects in the barometer as comparable to those in the thermometer; but there is the difference that in the barometer the expansion of the glass tube is of no significance, and the mercury column is not confined at its lower extremity, as in a thermometer-bulb. On

the other hand, heating a barometer expands the adjacent scale on which the height is read, and also increases the pressure of the air which must always be present, to some extent, in the Torricellian "vacuum." It was, however, of no use correcting barometers for thermal expansion until the above-mentioned arbitrary discrepancies in their indications had been removed. Cassini de Thury, following a practice of Du Fay, had noticed that when mercury was boiled in a number of barometer tubes, and these were then all inverted and plunged in a mercury trough, the columns all stood at the same height (*Mém. de l'Acad. Roy. des Sciences*, 1740). But this observation was ignored until De Luc, upon boiling the mercury in his barometers to render them phosphorescent, observed for himself that the anomalous differences in the heights of their columns largely disappeared. This treatment expelled the air and moisture which were dissolved in the mercury, or which adhered to the walls of the tube, and which would otherwise have risen into the Torricellian vacuum. Amontons supposed this air to percolate through pores in the glass, and Homberg thought it came from the spirits in which the new tubes were washed. De Luc compared the performance of boiled and unboiled barometers by placing several of each in a cold room whose temperature was gradually raised. He found that the boiled barometers gradually rose in concert with one another, while of the others one stood still, and the others sank by various amounts. When the room was cooled again, only the boiled barometers returned to their initial levels. From observations of these latter instruments when gradually heated, De Luc concluded that a rise of temperature from the freezing-point to the boiling-point of water would produce a rise of six lines in the barometric height at normal pressure, or proportionately less for smaller ranges of temperature. De Luc chose 12° on his thermometric scale (of 96° range) as the zero from which to reckon temperature corrections; these could then be systematically applied to barometric readings taken simultaneously at high and low stations, with due regard to the fact that different corrections must in general be applied to mercury columns of different heights, for purposes of comparison, even if they are at the same temperature. De Luc took precautions against errors in locating the upper and lower extremities of the mercury column against the scale of the barometer, arising from parallax or from the conformation of the mercury surface. He was also aware that, the more constricted the bore of a barometer tube, the lower the level to which the mercury rises above its external surface. In some experiments on a portable barometer (consisting of a U-tube with the longer limb sealed and the shorter limb open to the air) he found that constricting the area of the free mercury surface had the

effect of increasing the height of the column of mercury which the air supported. The same effect was produced by increasing the area of the enclosed surface; e.g., by blowing the top of the barometer tube into a bulb. Nor did De Luc overlook the errors which might arise from the use of faulty scales. When all his corrections had been applied, however, he still found some unaccountable discrepancies (of the order of one-sixteenth of a line of mercury). These, he thought, must be due to imperfections in the tubes employed. De Luc closes his section on the barometer, for the time being, with some hints on the management of the instrument during expeditions. It is most important that the instrument should be vertical when read, and the only means of ensuring this in mountainous country is by the use of a plumb-line. Since the indications of the barometer require correction for temperature, and since, in determining heights barometrically, account has to be taken of the temperatures of air-columns, De Luc was naturally led to attempt improvements in the construction and use of the thermometer, to which an important chapter of his book (Part II, Ch. 2) is devoted.

De Luc found the thermometers of his day in as unsatisfactory a condition as the barometers, owing to the diversity as well as to the technical defects in the methods of their construction. He saw the necessity of choosing the best method of manufacture, of adopting it universally, and of forsaking all other methods; and his researches were directed to this end. Fluids, he holds, are the best thermometric media because they expand appreciably on being heated, and can be made to show their expansion in fine tubes. But each fluid expands in its own fashion, and the conventional selection of one fluid is the first necessity. The ideal fluid would be one in which equal changes of volume were caused by equal increments or decrements of heat. But De Luc considered that the absolute quantity of heat in a body would probably always be unknown to us, and hence we could have no true zero for the thermometric scale, and could only measure quantities of heat added to a certain fixed quantity. After a careful experimental comparison and tabulation of the thermal expansions of several typical liquids—aqueous, oily, and spiritous—De Luc concludes that "Mercury is, of all liquids hitherto employed in the thermometer, the one which most exactly measures differences of heat by differences in its volume" (Vol. I, p. 285). Its expansion seems free from anomalies at the limits of its range of fluidity. In contrast, water, near its freezing-point (which is inconveniently high), actually *expands* with diminution of heat, while spirit of wine is irregularly dilated near its boiling-point. The factors producing these anomalies must presumably be active over the whole range of the liquid state, disturbing the tendency of the liquid to expand

uniformly as its heat-content is steadily increased. De Luc seems to
have been the first to recognize that the reversal of the contraction
of water into an expansion just above the freezing-point was an
actual property of water, and not a mere appearance due to the more
rapid contraction of the containing vessel. Other reasons enumerated
by De Luc for employing mercury as a thermometric liquid are, that it
is responsive to heat, expands appreciably, is easily freed from
dissolved air, and has a high boiling-point. Moreover, all mercury
thermometers expand according to the same laws, whereas alcohol
thermometers, for example, are appreciably affected by differences
in the concentration of the spirit, as is proved by DeLuc's table showing
the expansion of spirits of a number of different concentrations.

The thermometric fluid having been selected, the graduation of
the instrument is next considered. A method of graduating a
thermometer absolutely between a pair of terminal points had been
suggested to De Luc by Le Sage, several other experimenters of the
period recommending a similar procedure. The method consisted in
mixing quantities of water at different known temperatures and in
known proportions, calculating what the temperature of the mixture
should be, and taking this to be the correct reading of a thermometer
immersed in the mixture. De Luc performed experiments on these
lines, and he satisfied himself that the expansion of mercury approxi-
mates more closely than that of other fluids to the ideal, though even
here the expansion corresponding to a given increment of heat is
the greater the higher up the scale it occurs. As a result of his
experiments, De Luc was able to tabulate increments in the readings
of the mercury thermometer against increments of actual heat
(*chaleurs réelles*), and vice versa, and he did the same, in less detail, for
other typical fluids. The reckoning of degrees of heat by the con-
ventional division of the stem of a thermometer into equal parts by
length, could, De Luc concluded, be retained without sensible
error. Fixed points are next required to serve as the basis of gradua-
tions. The use of ice and boiling water to furnish these fixed
points, proposed by Renaldini in 1694, had become fairly general
at the time when De Luc was writing. The thermometers then
mostly in use were those of Réaumur and Fahrenheit. The former
divided the range between freezing-point and boiling-point into
80 degrees, after the expansion of 80 parts in 1000 undergone
by the spirit in rising from the freezing-point of water to *its own*
boiling-point, the eightieth degree being identified, however, in
course of time, with the boiling-point of *water* (*Recherches*, Vol. I,
p. 352). This was a source of subsequent confusion which De Luc
cleared up in the course of a systematic comparison of Réaumur's
scale with those of his own alcohol and mercury thermometers.

Réaumur determined his lower fixed point with the aid of a freezing-mixture surrounding the water-bath in which the bulb was placed; when this water froze, the tube was filled with spirit up to the zero-mark. But if the water was allowed to freeze solid, the mixture might chill the spirit considerably below zero, and Réaumur's instructions would not suffice to indicate the precise point required. De Luc's repetition of Réaumur's experiment seemed to suggest that the zero must have been fixed too low: when freezing had begun, but the upper part of the bulb was still free from ice, the thermometer already stood at $-3\frac{1}{2}°$, and it had reached $-5\frac{1}{2}°$ before the water was entirely frozen. Fahrenheit's lower fixed point was more likely to be definite and reproducible (see the author's *History of Science . . . in the Sixteenth and Seventeenth Centuries*, 2nd edn., p. 90). But convenience in practice had led to the use of Fahrenheit's freezing-point (32°) as the lower fixed point, and its determination was thus subject to the same uncertainties on Fahrenheit's scale as on Réaumur's. De Luc's method of fixing the freezing-point (since generally adopted) was to surround the bulb of the thermometer with pounded ice and water; his upper fixed point was provided by water in a steady state of ebullition, account being taken of the influence of the atmospheric pressure upon the boiling-point, as indicated by Fahrenheit (*Phil. Trans.*, 1724, p. 179). Compared with the choice of a single thermometric fluid, and of universal fixed points of temperature, the precise mode of graduating the interval between these points seemed to De Luc to be of secondary importance. The scales of Fahrenheit and Réaumur should, he thought, each be retained in the countries which had become accustomed to it, for general use.

De Luc's laboratory method of manufacturing thermometers is that now mainly followed. In testing a tube for uniformity of bore, he followed Nollet's procedure of passing a short thread of mercury down the tube from one end to the other, and measuring its length, in successive positions, with compasses. The length should remain sensibly constant, and fine capillary tubes are best as the bulbs need not then be large. De Luc proves Durand's formula giving a convenient value for the diameter of the bulb in terms of the length and bore of the tube and the number of degrees into which it is to be divided. In order to introduce mercury into a thermometer, De Luc fixed a reservoir to the open end of the tube; the bulb and tube were warmed so as to expel air, and, as they cooled, clean mercury, poured into the reservoir, was sucked down into the tube. After the bulb had been nearly filled by repeated heating and cooling, the mercury contained in it was boiled, all the air was expelled, and the tube was ultimately filled with mercury; the excess of mercury was

then expelled from the tube, and the open end was hermetically sealed. One advantage of De Luc's procedure was that the space above the mercury column was practically a vacuum. The fixed points were next marked by means of varnished threads attached to the stem—the boiling-point first, and then the freezing-point. The thermometer, when finished, was mounted on a base of some material whose variations of length with changes of temperature and humidity, and whose capacity for retaining heat, were as little as possible. Deal is recommended for this purpose. The scale-divisions on the base of the instrument were extended to pass behind the tube; this enabled errors of parallax in reading the temperature to be avoided, since all the divisions appeared bent by refraction except that which was directly opposite the eye. De Luc concludes with instructions for the manufacture of improved alcohol-thermometers for everyday use, by a procedure within the reach of craftsmen of average ability.

A long appendix to De Luc's second volume describes his researches on the variation of the temperature of boiling water with altitude above sea-level, especially in its bearing upon the correct method of graduating thermometers. Some isolated observations of this nature had already been made by L. G. Le Monnier in the Pyrenees (*Mém. de l'Acad. Roy. des Sciences*, 1740). But De Luc had noticed that the fall in the boiling-point was not simply proportional to the fall in pressure.

To obtain fresh light on this point, he constructed an extremely sensitive thermometer (Illustr. 129, Fig. 1), whose range of expansion between the freezing- and the boiling-points covered nearly the whole of its stem, and which was provided with a micrometer. This consisted essentially of a brass plate *g* which could be moved up and down at right angles to the stem by turning the screw (*de* in Fig. 2) by means of the handle *f*. When set at the exact level of the mercury, the brass plate enabled the temperature to be read off with great precision by means of an index travelling over a scale at the side. This scale measured whole turns of the screw, fractions of a turn being shown by a pointer which moved over a dial as the screw-handle, to which it was attached, was turned. The equivalent of one turn in terms of a degree of temperature had to be ascertained, and De Luc found that he could read to one-four-thousandth part of the interval between freezing-point and boiling-point. In order to secure uniformity in his experiments, De Luc constructed a special copper boiler in which the water was always boiled upon a portable stove (Fig. 3). Any water which might boil over was caught in a lip and conducted into the small vessel at the side. The thermometer was hung from the lid of the boiler (Fig. 4) by means of a strip of
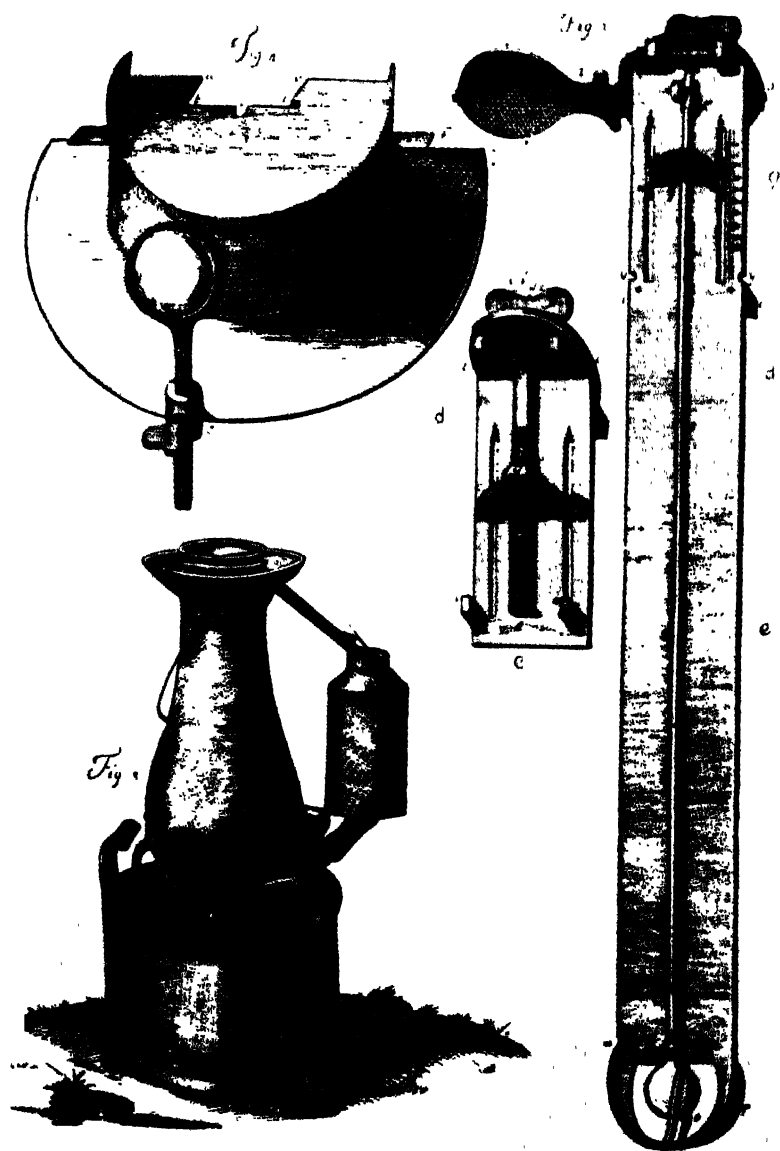
De Luc's Thermometer

brass *f* which was passed through slots in two projections *i*, *i*, at the back of the instrument. This lid only partly covered the mouth of the boiler; it was intended to shield the tube of the thermometer from steam (which would make it difficult to read the level of the mercury) without, however, raising the temperature by confining the steam. Readings were taken with the aid of the lens attached at *c*.

De Luc gives a graphic account of the expeditions which he and his brother undertook in 1765–70 among the unexplored mountains and glaciers of Faucigny, near Geneva, where, despite many mishaps, joint observations of pressure and boiling-point were made at various altitudes. The results of these observations convinced De Luc that the boiling-point decreased in a more rapid proportion than the corresponding barometric pressure. Having satisfied himself that this result was not due to differences in the purity of the water used or in the temperature of the surrounding air, he tabulated his observations so as to reveal any simple law connecting the pressure and the boiling-point. The results seemed to show that, if the pressures formed an arithmetical progression, the corresponding differences in the temperature at which water boiled followed a harmonical progression, within the limits of experimental uncertainty. This could not be attributed to the peculiarities of the mercury in the thermometer, the relation of whose changes of volume to the underlying differences of heat De Luc had already investigated and now allowed for. He was thus enabled to calculate the correction to be applied in marking the boiling-point on a thermometer at any given atmospheric pressure, though he was less successful in his search for a general physical explanation of his law.

Having pointed out the pitfalls into which previous investigators must have fallen, and having done much to remove the pitfalls, De Luc devotes most of the third Part of his book to describing in great detail the design of the portable barometer which, after several trials, he finally adopted as most suited to his purpose (Illustr. 130). It consisted essentially of a ∪-shaped tube in two parts which were connected through a cock in the shorter arm of the

Illustr. 130.—De Luc's Portable Barometer

tube. The longer arm was closed, and the shorter open, at the top. The whole was enclosed in a deal box. When the instrument was to be transported to a distance, especially if the way lay over uneven ground, the longer arm was completely filled with mercury, the cock was closed, and whatever mercury was left in the shorter arm was emptied out. The danger of damage from violent agitation of the mercury in the longer arm was thus avoided. A thermometer was also mounted on the base of the instrument so as to admit of correcting the barometric reading for temperature. The whole instrument in its box was carried upside down like a quiver, and was set up vertically, with the aid of a plumb-line, upon a tripod, when observations were to be made with it.

In Part IV, De Luc at length addresses himself to his ultimate task of establishing a rule connecting barometric pressure with altitude. In order to obtain the precise information that he needed, De Luc selected in the mountain of Salève, near Geneva, a number of stations whose relative heights he determined independently by triangulation with a base-line and a telescopic quadrant, and by levelling. In subsequently measuring the heights of precipices by plumb-lines, he guarded against errors arising from the stretching of the lines under the weights which they carried, by measuring them while still under tension. The hundreds of observations upon which De Luc's law was based, were made in concert on the mountain and in the plain. On each day when observation was possible, the barometer and the thermometer were read every quarter of an hour from morning till evening, the observations in the plain being in the charge of De Luc's father. De Luc repeatedly compared the fluctuations occurring throughout a whole day in the readings of the two barometers. He found that the readings did not show parallel changes, the difference between them varying appreciably in the course of a day even when allowance was made for the different temperatures of the mercury columns. He attributed these anomalies to the convectional air-currents set up by the heating of the plain under the Sun's rays. In analysing his results, De Luc had to disentangle the combined effects of changes of temperature and of the gradual falling off in pressure with altitude above the Earth's surface. First he formed, from his observations on Salève, an empirical table showing the relation of pressure to elevation *on the average*, and without regard to temperature. Then he compared the difference of altitude of each pair of stations, as determined barometrically with the aid of this table, with the difference as already determined by measurement. By tabulating the differences between the estimates obtained in these two ways, against the mean temperatures of the air-column intercepted between the two stations (taken as the mean

of the temperatures of the base and the summit of the column), he deduced, for each pair of stations, the difference in calculated altitude due to one degree of difference of mean temperature. By comparing the results for all the stations he obtained a first approximation to the general temperature correction to be applied. He found, however, that his results needed adjustment according as the barometric pressure in the plain at the foot of the mountain was higher or lower than its usual value. De Luc's corrected table showed conformity of structure with the theoretical tables which had been based upon Boyle's Law. He was thus able to express his rule in the form anticipated on theoretical grounds by Halley in the seventeenth century, namely, that the height of any vertical column of the atmosphere is proportional to the difference of the logarithms of the pressures at its upper and lower extremities, and he formulated the rule that "at a certain temperature, the difference of the logarithms of the heights of the mercury, gives immediately, in thousandths of a fathom, the difference in height of the places where the barometer was observed" (Vol. II, p. 84). In order to determine what this "certain temperature" was, and what corrections must be made when the observed temperature differed from it, De Luc picked out from his recorded observations those instances in which the differences of the logarithms of pressure most approximately gave the (previously measured) differences in altitude of the respective stations, in thousandths of a fathom; and he found that the mean temperature prevailing when these observations were made had been 16¾° on his eighty-degree thermometer. He then grouped his other observations according to the stations and the temperatures at which they had been made, and having noted the discrepancies between corresponding differences of altitude (1) as calculated from the above logarithm-rule, and (2) as obtained by direct measurement, he deduced the correction to be applied, in such calculations, for each degree of difference of actual temperature from the normal temperature of 16¾°, at each station. Next, comparing the sets of corrections for temperature at the several stations, De Luc found them to be approximately proportional to the altitudes of the respective stations above the common base from which they were reckoned. Thus the corrections were found to be jointly proportional to the altitudes of the respective stations and to the number of degrees by which the readings of the thermometer differed from the standard temperature. There were still some outstanding discrepancies, however, which this system of observations did not cover. De Luc tabulated all his observations anew, for each station, in such a manner as to bring out such correlations as might exist between these discrepancies and other relevant circumstances. He noticed that observations taken

about sunrise consistently led to under-estimates of the altitude of the place of observation. He was inclined to attribute this to the agency of the east wind blowing at dawn. He thought it best to leave out of account observations made at this time of the day, and he accordingly modified his temperature-corrections, which had, in part, been based upon them. In its final form, his scale of corrections decreased progressively with rise of temperature, the correction per degree of difference from the standard temperature being $\frac{1}{215}$ of the height of the station *as given by the logarithm-rule*. In order to facilitate the reduction of his observations, De Luc employed a special thermometric scale—rather a favourite procedure of his in such circumstances. He then expressed the difference, in fathoms, of the altitudes of the two stations by the formula:

$$\frac{(\log c - \log b) \pm \dfrac{(\log c - \log b) \times a}{1000}}{1000}$$

where $a =$ number of degrees in excess, or defect, of standard
temperature (on the special scale);
$b =$ height of mercury in barometer at upper station;
$c =$ height of mercury in barometer at lower station
(Vol. II, p. 166).

This Part of the work concludes with tables showing with what degree of consistency the barometrical estimates of the altitude of each of the fifteen stations agreed with one another, and with the altitude as obtained geodetically. De Luc was naturally anxious to prove that his rule held good for other localities than that in which all his fundamental observations had been made. He therefore verified it by making barometric observations on such eminences as the cathedrals of Geneva and Turin and the lighthouse at Genoa, and he compared the differences of altitude deduced from these observations, by the application of his formula, with that obtained by the use of a plumb-line, or by levelling. He obtained good agreement in all cases; for example, the barometric method gave the height of the lighthouse as 221 feet 1 inch, as against the direct measurement of 222 feet 11 inches. Other sets of observations in the Alps were designed to show the *consistency* of the results of repeated applications of De Luc's method. Estimates of height made by De Luc at sea-level satisfied him that his rule held good there also, although deduced from observations in which the zero of altitude was at an arbitrary elevation above sea-level. De Luc also reviews the recorded observations of Bouguer in Peru and of La Caille at

the Cape of Good Hope, which, he considered, confirmed, for what they were worth, the generality of his own formula.

De Luc proceeds next to review some of the difficulties remaining to be overcome in this domain (e.g., residual imperfections of barometers, dissimilarity in the respective laws of expansion of mercury and of air, uncertainty about what we should call the temperature-gradient in air-columns, and so forth). He surmised that the formulae for determining heights barometrically would ultimately have to be extended so as to take account of the concentration and the temperature of the vapours contained in the air-columns under measurement. Meanwhile, he suggested, the effects of such small disturbing factors could be largely eliminated by observing the barometer every fifteen minutes over a number of hours, and taking the mean of the readings. But if there was time for only one observation, this was best made in the morning, when the Sun had completed one-fifth of his course above the horizon, the atmosphere being then in its most tranquil and pure condition. The rule for determining differences of altitude barometrically could not be expected to hold accurately unless the horizontal distance between the two observing-stations was small. If, however, the barometer was to be used for comparing the altitudes of stations throughout a district, De Luc thought the safest procedure would be always to read the barometer at the same hour, and to compare the observations made *en route* with corresponding readings taken simultaneously at a fixed station in open country. De Luc tabulates the results of his barometric levelling of a number of Alpine routes. He proposed that, by means of a network of observing stations, the whole of Europe should be levelled. He thought that an extension of this system along all the coasts of the world might, in proper hands, lead to interesting discoveries concerning differences of sea-level and the causes of winds and currents; perhaps it might even give information about the figure of the Earth.

De Luc attached a level to his portable barometer, and he was thus able, by a combined use of the two instruments, to estimate the altitudes of various landmarks surrounding each observing station, and to save himself many laborious ascents. With his brother, he estimated the height of Mont Blanc by noting a certain landmark on the mountain-side which was level with the top of the Glacier de Buet (whose altitude was known barometrically), and subsequently measuring from a station near Geneva (of known altitude) the angular elevations of the landmark and of the summit of the mountain. The altitudes of these two points above the observing station were then proportional to the tangents of these elevations (subject to a correction for the greater remoteness of the summit);

and from the known altitude of the landmark that of the mountain itself was deduced by proportion to be 14,346 feet above sea-level. Later, in 1787, H. B. de Saussure ascended Mont Blanc and observed the pressure and temperature at the summit. Comparison of his readings with others simultaneously made at Geneva enabled an independent estimate of the height of the mountain to be obtained, namely, about 15,700 feet (see his *Voyages dans les Alpes*, Vol. IV, p. 192).

The concluding chapters of De Luc's work (Part V) deal briefly with the application of his discoveries about the atmosphere to the problems of determining the specific gravity of the air at standard temperature and pressure, of estimating the extent of the atmosphere (or rather, as that is impossible, of finding the height above the Earth's surface at which the pressure falls to some small assigned value), and lastly, of formulating the relation between astronomical refraction and the temperature and pressure of the atmosphere. The existence of any such relation had frequently been denied earlier in the eighteenth century. But De Luc connects refraction with *temperature* by means of the equation:

$$\frac{1000a}{1000 \pm 2c}$$

where a is the mean refraction, b is the actual refraction required, and c is the number of degrees excess of or defect of temperature, on a thermometric scale specially chosen to facilitate the calculation. Following Halley, he assumed refraction proportional to the *pressure*, or density, of the air at the Earth's surface. He thought that the presence of vapours might have a further important effect upon the refraction, and that a hygrometer would be of service in the observatory; but he could not make any further progress in this direction.

De Luc's formula for the barometric determination of altitude was revised by Laplace, allowing for variations of gravity with altitude and latitude (*Mécanique Céleste*, Bk. X, Ch. 4), and by Sir George Shuckburgh (*Phil. Trans.*, 1779, p. 362).

D. THE STUDY OF NORTHERN LIGHTS

There is evidence that the "Northern Lights" were known in classical times, though they can scarcely ever have been visible from the Mediterranean. Even in such latitudes as those of the British Isles, auroral displays are sufficiently rare, and no consistent account was taken of them until comparatively modern times. A clear and unmistakable description of the aurora borealis is found in a Scandinavian work of the thirteenth century, but throughout most of

Europe displays such as those of the sixteenth century were generally
mistaken for comets, and though described in popular literature of
the time were soon forgotten. In the seventeenth century the
phenomenon was beginning to be known under the name aurora
borea : this term seems to have been introduced into scientific literature
by Gassendi, and it developed into aurora borealis. The scientific
study of the aurora, however, may be said to have begun with the
eighteenth century. A display in March 1707, visible all over
central Europe, awakened much interest, and a clear summary of the
general properties of the phenomenon, based upon long familiarity
with it, was given by H. Vallerius in his *Exercitium philosophicum
de Chasmatibus* (Upsala, 1708), though this work long remained
unknown. Vallerius attributed the phenomenon to the reflection by
ice-crystals in the upper atmosphere of sunlight coming from below
the horizon, somewhat as halos are formed round the Sun and Moon.
This explanation seems to have been fairly general at the time,
though William Whiston, writing of the display of 1716, explained it
by reference to sulphurous exhalations rising from the Earth in the
north, and only prevented from producing a thunderstorm by the
arctic cold. This display of March 16–17, 1716, is noteworthy,
not only because it was visible throughout Europe and North
America, but also because it was the means of bringing the
phenomenon to the notice of Edmond Halley, one of the most
experienced scientific men of the time.

Halley published in the *Philosophical Transactions* (1716, p. 406)
a graphic description of the spectacle (which he saw with his own
eyes), and a critical discussion of its causation. His paper stands out
from the other accounts which were everywhere forthcoming, and
virtually marks the beginning of the scientific treatment of the
subject. Halley missed the first sudden outburst of the aurora, but
he was able to watch its later stages for hours, and he supplemented
his description with an illustrative plate. In his paper he notes
especially the glowing cloud-like patches near the horizon, and the
ruddy coruscating beams shooting upward towards the zenith,
forming there what he was the first to call a corona ; and he emphasizes
the fact that many of the luminous "vapours" appeared in the
southern part of the sky, and thus in the heart of the Earth's shadow-
cone, so that they could not have owed their light to the Sun.
Halley recognized also that the apparent directions of the streamers,
perpendicular to the horizon, and their convergence towards the
zenith, were consequences of their rising perpendicularly to the
Earth's surface at their respective points of origin, and, further,
that the pyramidal shapes of the streamers were likewise the effects
of perspective. At first Halley thought that the phenomenon might

be due to "the Vapour of Water rarified exceedingly by subterraneous Fire, and tinged with sulphureous Steams; which Vapour is now generally taken by our Naturalists to be the Cause of Earthquakes." An historical review which he gives of previous recorded displays of this nature suggested that they occur in groups widely separated in time, and this of itself suggested comparison with the similar characteristic of earthquakes. But this hypothesis could not explain why such displays are confined to the northerly regions of the sky; nor was it compatible, Halley thought, with the enormous scale on which they occur, as proved by their extensive visibility. Hence he preferred to link his explanation on to the theory of terrestrial magnetism. The Earth, being a magnet, must be the centre of such a circulation of magnetic effluvia as surrounds a *terrella* or spherical lodestone. The form of this circulation is revealed by the arrangement of steel filings in any axial plane of the *terrella*, the effluvia evidently entering at one pole and leaving at the other. We are to suppose, then, "that this subtile Matter . . . may now and then, by the Concourse of several Causes very rarely coincident, and to us as yet unknown, be capable of producing a small Degree of Light; . . . after the same manner as we see the Effluvia of Electric Bodies by a strong and quick Friction emit Light in the Dark: to which sort of Light this seems to have a great Affinity." This hypothesis Halley found to be consonant with the various optical effects presented by the aurora. He was tempted to identify his luminous effluvia with the material composing nebulae and the tails of comets, and with a certain luminous substance which, he had formerly supposed, might exist for the purpose of lighting up the interior spaces of the Earth, and so of making them habitable. Halley recommended that in future displays of this kind observers should note at the end of each half-hour the situation, against the background of the sky, of the salient features of the spectacle, so that, by subsequent comparison of such observations, the height of the aurora might be estimated. Halley himself does not use the term "aurora" in his paper, but writes of the "Lights seen in the Air," the "Meteor," etc. The term aurora borealis was, however, used by Edmund Barrell and Martin Folkes in their descriptions (*Phil. Trans.*, 1717, pp. 584, 586) of displays in the early part of the following year; both draw attention to the westward displacement of the vertex of the auroral arch from the geographical north. Halley also seems to have observed this displacement, and to have recognized it as a significant cross-link with the properties of terrestrial magnetism. In the Journal of the Royal Society (quoted by L. A. Bauer in *Terrestrial Magnetism*, Vol. XVIII, No. 3) there are the following Minutes:

November 10, 1726. "Dr. Halley related a material circumstance observed in the late aurora borealis, which serves to confirm him in his former opinion, that the magnetical effluvia of the Earth are concerned in the production of the phenomenon, and that was from the situation of the luminous arch in the north and the tendency of the motion of the *striae*, both which seemed to have a dependence upon the magnetical virtue. He said the arch was highest in that place where it crossed the magnetical meridian, and the *striae* had a motion with an inclination like that of the magnetical dipping needle."

November 21, 1728. [Referring to a letter from Derham about an aurora, Halley said that] "it has ever been observed since he first took notice of it that the centre of the luminous arch and black basis always lies in the magnetical meridian, and seems to change its place in the horizon as that alters, as far as can be observed." At the end of the eighteenth century, as already stated, John Dalton (*Meteorological Observations and Essays*, 1793) pointed out that the streamers of the aurora are parallel to the dipping needle, and that the vertex of the aurora lies in the magnetic meridian. Frequent accounts of auroras occur in subsequent numbers of the *Philosophical Transactions*. There are also reports of alleged *aurorae australes;* one of these was certainly observed by Antonio de Ulloa from near Juan Fernandez, about 1740.

Attempts to account for the occurrence of auroras continued to be made during the eighteenth century. Thus J. J. D. de Mairan, in his *Traité physique et historique de l'Aurore Boréale* (Paris, 1733) attributed the phenomenon to an extension of the Sun's atmosphere, which at times, he supposed, enveloped the Earth, and blended with our atmosphere. The zodiacal light was similarly accounted for. Mairan also attempted some estimate of the height of the auroral disturbances. John Canton (*Phil. Trans.*, 1759, p. 398) confirmed the connection between auroral displays and irregular variations of the compass-needle, which had been discovered by Hjorter (1741), and by other Scandinavian observers. Canton supposed both these effects to arise from exceptional heating of portions of the Earth's surface by subterranean causes. Euler thought that the auroral streamers were portions of the Earth's atmosphere, shot out by the action of the Sun, like the tails of comets (*Mem. of Berlin Acad.*, 1746). The electrical nature of the aurora was admitted later in the eighteenth century; it is nowadays regarded as an electrical discharge in the atmosphere due to ionization caused by particles shot out by the Sun.

(See G. Hellmann, *Beiträge zur Geschichte der Meteorologie*, Berlin, 1914, etc.)

METEOROLOGICAL INSTRUMENTS

THE meteorological instruments in use during the eighteenth century were (1) the rain-gauge, for measuring the amount of rain-fall; (2) the thermometer, for measuring degrees of temperature; (3) the barometer, for measuring variations of atmospheric pressure and the heights of mountains, etc.; (4) the anemometer, for measuring the force, direction, and velocity of winds; and (5) the hygrometer, for measuring the amount of moisture in the air. All these instruments had been invented and were in use before the dawn of the eighteenth century. The rain-gauge, indeed, remained unchanged, and so need not be considered here. But the other four instruments were improved in important respects as the result of a searching investigation into the principles involved in their construction and use. The barometer and the thermometer have been considered already in the preceding chapter, in the account of De Luc's barometric and thermometric study of the atmosphere. The story of the improvements in these instruments is so intimately connected with De Luc's meteorological researches as to be almost inseparable from them. It is unnecessary to add anything here about the barometer; but something must still be said about the more familiar types of thermometer before proceeding to give an account of the anemometers and hygrometers in use during the eighteenth century.

A. THERMOMETERS

THERMOSCOPE AND THERMOMETER

The development of the thermometer into a standard scientific instrument, begun by Galilei early in the seventeenth century, was practically completed during the eighteenth century. The earliest instrument of this type had been the *thermoscope*, consisting essentially of an inverted flask dipping into water, and merely serving to show changes in the temperature of its surroundings by the rise or fall of the level of the water in the neck of the flask caused by the contraction or expansion of the imprisoned air. It was subsequently found, however, that the volume of this air was appreciably affected by changes in the barometric pressure, and the thermal expansion of liquids (such as water or alcohol) came to be utilized instead. The thermoscope was next made to give quantitative indications, and was thus converted into a *thermometer*, by affixing to it a scale upon which the height of the liquid column could be read as representing

so many "degrees" of heat or cold. These degrees were at first purely arbitrary in value, and they were reckoned from an arbitrary zero. But in order to compare the indications of different thermometers under different circumstances, it was desirable to adopt a universal scale of temperature. It was proposed to secure this by reckoning temperatures on all thermometers from a "fixed point" corresponding to a certain definite temperature, such as the temperature of freezing water, which could be easily produced when desired. But even when such a fixed point had been selected, the magnitude of the degree, or unit of the scale of temperature, remained undetermined. Before the end of the seventeenth century, however, this problem had been solved in principle by the suggested use of *two* fixed points (such as the freezing-point and boiling-point of water), and the division of the interval between these points into a conventional number of equal parts. This procedure seems first to have been systematically put into practice in the eighteenth century by Fahrenheit; it was he, moreover, who established the use of mercury as a thermometric fluid, its previous employment for that purpose having been only occasional and experimental.

FAHRENHEIT

Daniel Gabriel Fahrenheit was born in 1686 in Danzig, the son of a German merchant. He was sent to Holland to learn business, but his scientific tastes prevailed, and he eventually established himself as a maker of scientific instruments in Amsterdam, where he died in 1736. Fahrenheit visited England, and he was elected a Fellow of the Royal Society; his papers on thermometry were published in Latin in the *Philosophical Transactions*. It appears that Fahrenheit at first employed alcohol as his thermometric fluid; but by 1721 at latest he had constructed his first mercury thermometer; it was intended primarily for the purpose of confirming the observation made by Amontons (and by others before him) that water boils at a constant temperature. He also employed it in the determination of the boiling-points of other liquids, such as alcohol, nitric acid, oil of vitriol, etc., of which he gives the specific gravities taken at 48° on his scale, this temperature (he explains) lying midway between the greatest cold obtainable by a mixture of water, ice, and salt, and that of the blood of a healthy man (*Phil. Trans.*, 1724, pp. 1 ff.). From the particulars given in his account of some further experiments, on the supercooling of water (*Ibid.*, pp. 78 ff.), it appears that Fahrenheit marked the zero on his thermometers by immersing them in the above mentioned freezing mixture (he does not specify in what proportions the ingredients were mixed). See, however, the author's *History of Science . . . in the Sixteenth and Seventeenth Centuries*,

2nd edn., p. 90, for the origin of Fahrenheit's scale. On this scale the melting point of ice was 32°, and the temperature of the healthy human body (obtained by inserting the thermometer in the mouth or in the armpit) was 96°. When this scale was extended so as to include the boiling-point of rain-water, the thermometer registered 212°; but Fahrenheit did not use the boiling-point as an experimentally determined "fixed point" of his scale. For everyday purposes Fahrenheit graduated his thermometers only up to 96°, and further experiments convinced him that the boiling-point of water varies slightly with changes in the atmospheric pressure. He was thus led to the invention of the hypsobarometer or hypsometer. This instrument was essentially a thermometer; but it was so constructed and graduated that when it was immersed in boiling water the top of its alcohol or mercury column indicated directly upon an adjacent scale the atmospheric pressure under which the water was boiling. Fahrenheit's scale, freed from the vagueness which vitiated his definitions of its fixed points, has established itself for most practical purposes in English-speaking countries.

Illustr. 131.—Fahrenheit's Hypsometer

The cylinder AB was filled with alcohol or mercury, whose expansion in the tube BC measured the temperature on the scale *bc*. But when the instrument was placed in boiling water, the liquid expanded so as to fill the bulb CD and entered the tube DE, where its height served to measure, on the scale *de*, the atmospheric pressure at which the water boiled.

RÉAUMUR

The improvement of the thermometer was pursued on different lines, and in ignorance of the work of Fahrenheit, by the aristocratic French naturalist René Antoine Ferchault de Réaumur. Born at La Rochelle in 1683, Réaumur made a name for himself as a

mathematician, a zoologist, and an authority on industrial technology and on the natural resources of France. In 1708 he became a member of the *Académie des Sciences*, and he died in 1757. Réaumur's "rules for constructing thermometers with comparable graduations" are set forth in the *Hist. et Mém. de l'Acad. de Paris* for 1730 (pp. 452 ff.) and 1731 (pp. 250 ff.). Réaumur graduated his thermometers so as to make the successive degrees correspond to equal increments in the volume of the thermometric fluid, each increment representing a definite fraction (generally one-thousandth part) of the volume of the fluid at the freezing-point of water, which was the only fixed point he used for defining his scale. (This principle had been anticipated by Hooke in the middle of the seventeenth century, and by Newton at the beginning of the eighteenth century.) Réaumur rejected mercury as a thermometric fluid owing to its relatively low coefficient of expansion; he preferred to work with alcohol. He made his fundamental thermometers larger than was usual in his day. He took a glass bulb about 4·5 inches across into which was fused a glass tube about a quarter of an inch in diameter, initially open at the upper end (Illustr. 132, Fig. 1). In graduating the instrument he employed a small pipette (Fig. 2), the capacity of which served as his arbitrary unit of volume, and also several larger pipettes (Figs. 3, 4, 5, 10) and measuring-flasks (Figs. 6, 7) containing multiples of this unit. By repeatedly filling the pipette with water and emptying it down the thermometer tube, he filled the bulb and the lower part of the stem with 1000 parts of water. He then tied a thread round the stem to mark the level to which the water rose; this was to serve as the zero of the scale. The bulb and stem were then attached firmly to a plate (Fig. 8) upon which Réaumur proceeded to mark the graduations in the following manner. He filled the unit pipette with water (or, preferably, with mercury, which did not adhere to the glass), emptied it down the tube, and marked on the plate the level to which the water now rose with a horizontal line and a figure 1. He then emptied another pipetteful into the tube, and marked the new level with another horizontal line and a figure 2. This process was continued until the graduations had been extended as far up the stem as it was desired to carry them, each degree corresponding, as already stated, to a thousandth part of the volume of the liquid necessary to fill the bulb and stem up to the zero. Degrees below zero were obtained by filling the tube up to 0°, and then emptying out, with the aid of a measuring flask, say twenty-five parts of liquid; the resulting level was marked as 25° below zero, and the intervening graduations up to 0° were then obtained as before with the aid of the unit pipette. The water and mercury were next emptied out from the bulb and

stem, and the interior of the instrument was dried. Sufficient alcohol was then poured in to come up just to the zero when the bulb was immersed in a sheet-iron vessel containing water, and subjected to the action of a freezing mixture contained in an outer vessel. The open end of the thermometer tube was finally sealed hermetically, or by means of a mixture of wax and turpentine. Réaumur discusses at considerable length the question of how much air should be left

Illustr. 132.—Réaumur's Thermometer

in the sealed thermometer tube, and the anomalies which appear to arise from its presence; his ultimate conclusion is that the sealed tube should contain air in a moderately rarefied condition. The indications of a thermometer graduated in the manner above ' described depend upon the purity of the alcohol which it contains, since the purer the spirit the more expansive it is. Hence the importance of standardizing the spirit to be employed in such thermometers. Réaumur suggested, as the criterion of the quality

of a sample of spirit, the proportion in which it was found to expand when its temperature was raised from the freezing-point to the boiling-point of water. This test had the practical disadvantage that the spirit began to boil at a temperature considerably lower than the boiling-point of water (as Réaumur well knew, and as his critics were not slow to point out). Réaumur found, however, that he could, in some sense, determine the volume of a specimen of alcohol at the boiling-point of water by the following procedure: he enclosed the alcohol in a narrow-necked flask (Illustr. 132, Fig. 12), immersed this in boiling water, and, when it began to boil, removed it and noted the level of the spirit as soon as its ebullition ceased; he repeated this process, and observed that the level rose progressively up to a certain point, beyond which no further expansion would take place; he took this maximum volume of the alcohol as that corresponding to the boiling-point of water, and he adopted, as the spirit of standard purity with which his thermometers were to be filled, that which expanded from 1000 parts to 1080 parts by volume when heated from the freezing-point of water to the highest temperature which the spirit could acquire from boiling water without itself boiling. The boiling-point of water was thus employed by Réaumur, not to give an upper fixed point for his thermometric scale, but to standardize his thermometric liquid. In the so-called "Réaumur scale," however, as applied in the latter part of the eighteenth century to mercury thermometers, and still employed on the Continent, the melting-point of ice is taken as 0°, and the boiling-point of water, at standard pressure, as 80°. Réaumur's methods and ideas were criticized and amended by De Luc, whose important contributions to the perfection of the thermometer have already been considered.

CELSIUS

In 1742 the Swedish astronomer Anders Celsius (1701–44) of Upsala described his method of constructing a mercury thermometer (*Vetensk. Akad. Handl.*, 1742; *trad.* Kästner, Bd. IV, pp. 197 ff.). The scale of this instrument had two fixed points, of which the lower was obtained by surrounding the bulb with moist snow for about half an hour, and marking the level to which the mercury sank, while the upper fixed point was represented by the level attained when the bulb was lowered for several minutes into a teapot full of boiling water, kept in violent ebullition by means of glowing coals and bellows, the barometric pressure having its mean value at the time of the determination. The portion of the stem between these two fixed points was graduated into one hundred equal parts; but Celsius placed his zero at the boiling-point of water, and his 100°

division at the melting-point of snow, so that the readings increased *down* the scale, and negative numbers for temperatures below freezing-point were avoided. The mercury thermometer with these fixed points and with a centesimal graduation increasing *upward*, as in the now familiar Centigrade scale, seems first to have been
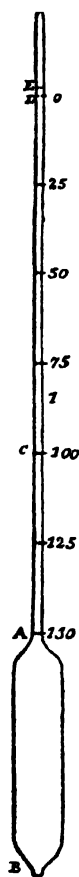
Illustr. 133.—Celsius' Thermometer
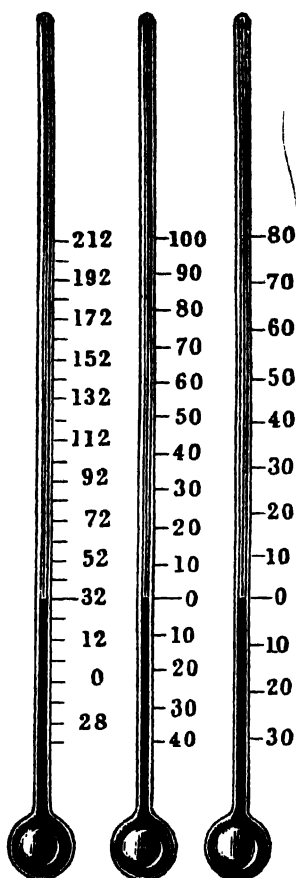
Illustr. 134.—The Three Thermometric Scales

introduced by Christin of Lyons in 1743, and was described by him in the local papers.

The three thermometric scales respectively associated with the names of Fahrenheit, Celsius, and Réaumur (though no longer now defined precisely as by their originators) are shown side by side for purposes of comparison in Illustr. 134.

(The writings of Fahrenheit, Réaumur, and Celsius on the thermo-
meter are published in German translations, with notes, in Ostwald's
Klassiker, No. 57. See also H. C. Bolton: *The Evolution of the
Thermometer, 1592–1743*, 1900.)

MAXIMUM AND MINIMUM THERMOMETERS

It is often a matter of interest to meteorological observers to know
the highest and lowest temperatures attained by the thermometer
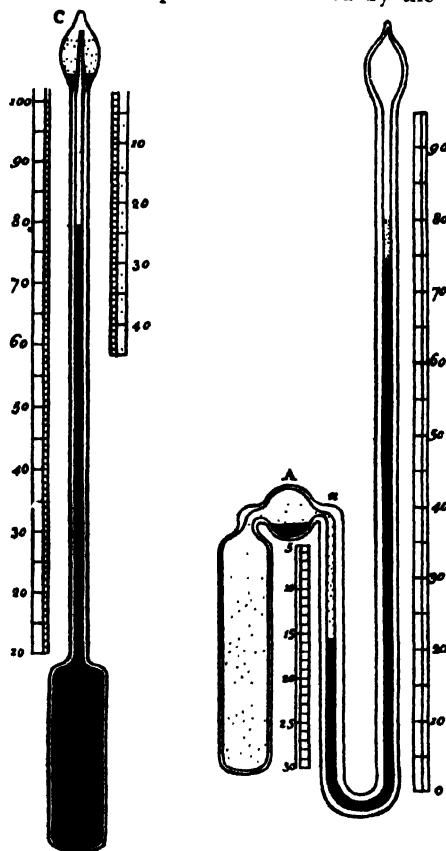
Illustr. 135.—Cavendish's Maximum and Minimum Thermometers

within a certain period of time, generally twenty-four hours. During
the eighteenth century instruments were invented for providing this
information without the necessity of constantly watching the indica-

tions of the thermometer; these devices were the earliest *maximum and minimum thermometers*, or *registering thermometers*. Numerous contrivances of this kind, many of them complicated and impracticable, were proposed in the latter part of the eighteenth century; of these we shall consider only a few typical instruments mostly working on principles which are still utilized in the mechanical registration of maximum and minimum temperatures.

A simple early example, in which the maximum and the minimum thermometers were separate instruments, was that devised by Lord Charles Cavendish (*Phil. Trans.*, 1757, pp. 300 ff.), and shown in Illustr. 135. The special feature of the maximum thermometer (figure on left) was that the top of the stem was drawn out in the form of a capillary tube which opened into a glass reservoir C. The cylindrical bulb and part of the stem of the thermometer contained mercury, the level of which showed the temperature in the ordinary manner on the scale to the left. Above the mercury was a column of alcohol; the reservoir C was also partly filled with this fluid. When the temperature rose and the mercury expanded, some of the alcohol was driven out of the stem into the reservoir; if the temperature then fell, an empty space appeared above the spirit in the stem, proportional in length to the fall of the thermometer from the highest temperature reached. "Therefore, by means of a proper scale, the top of the spirit of wine will show how many degrees it has been higher than when observed; which being added to the present height, will give the greatest degree of heat it has been at." The instrument was reset after an observation by tilting it until the alcohol in C covered the end of the capillary tube; the bulb was then heated until the spirit in the tube began to enter C; when allowed to cool, the spirit was sucked back from the reservoir into the stem, so as to fill its upper part. The reservoir C also contained some mercury to provide sufficient depth of the metal in case any should be expelled from the stem and should have to be replaced there by a similar process of suction. Cavendish's minimum thermometer (figure on the right) was somewhat like an inverted siphon-tube. The top of the longer limb was sealed, while that of the shorter entered the ball A which communicated with a large cylinder. The cylinder and the ball initially contained alcohol, while a thread of mercury extended from the top of the shorter limb of the tube to a point some way up the longer limb, where its level (or that of a short column of alcohol above it) showed the temperature of the surroundings on a scale. As the temperature fell the spirit in the cylinder contracted, and mercury ran over from the shorter limb into the ball, where it was trapped. If the temperature subsequently rose, the upper part of the shorter limb filled with a column of spirit

the length of which was proportional to the rise of temperature; and the reading of the mercury level in the shorter limb, on the adjacent scale, "will show how much the thermometer has been lower than it then is; which being subtracted from the present height, will give the lowest point that it has been at." To reset the instrument, it was tilted until the mercury in the ball covered the opening *n*; the cylinder was then heated, and the mercury was forced out of the ball. Cavendish suggested modifications of these instruments, especially with a view to their employment in the depths of the sea.

Cavendish's type of instrument, however, was superseded towards the close of the eighteenth century by thermometers with small movable indices operated by the rise or fall of the surface of the thermometric liquid, such as may still be seen in use. The prototype of all such instruments was the combined maximum and minimum thermometer of James Six (*Phil. Trans.*, 1782, pp. 72 ff.); its construction and mode of operation may best be described in the inventor's own words (see Illustr. 136): "Fig. 1. *ab* is a tube of thin glass, about sixteen inches long, and five-sixteenths of an inch in diameter; *cdefgh* a smaller tube with the inner diameter, about one-twentieth, joined to the larger at the upper end *b*, and bent down, first on the left side, and then, after descending two inches below *ab*, upwards again on the right, in the several directions *cde, fgh*, parallel to, and one inch distant from it. On the end of the same tube at *h*, the inner diameter is enlarged to half an inch from *h* to *i*, which is two inches in length. This glass is filled with highly rectified spirits of wine to within half an inch of the end *i*, excepting that part of the small tube from *d* to *g*, which is filled with mercury. . . . When the spirit in the large tube, which is the bulb of the thermometer, is expanded by heat, the mercury in the small tube on the left side will be pressed down, and consequently cause that on the right side to rise; on the contrary, when the spirit is condensed by cold, the reverse will happen, the mercury on the left side will rise as that on the right side descends. The scale, therefore, which is Fahrenheit's, beginning with o at the top of the left side, has the degrees numbered downwards, while that at the right side, beginning with o at the bottom, ascends. . . . Within the small tube of the thermometer, above the surface of the mercury on either side, immersed in the spirit of wine, is placed a small index, so fitted as to pass up and down as occasion may require: that surface of the mercury which rises carries up the index with it, which index does not return with the mercury when it descends; but, by remaining fixed, shows distinctly, and very accurately, how high the mercury had risen, and consequently what degree of heat or cold had happened." Fig. 2
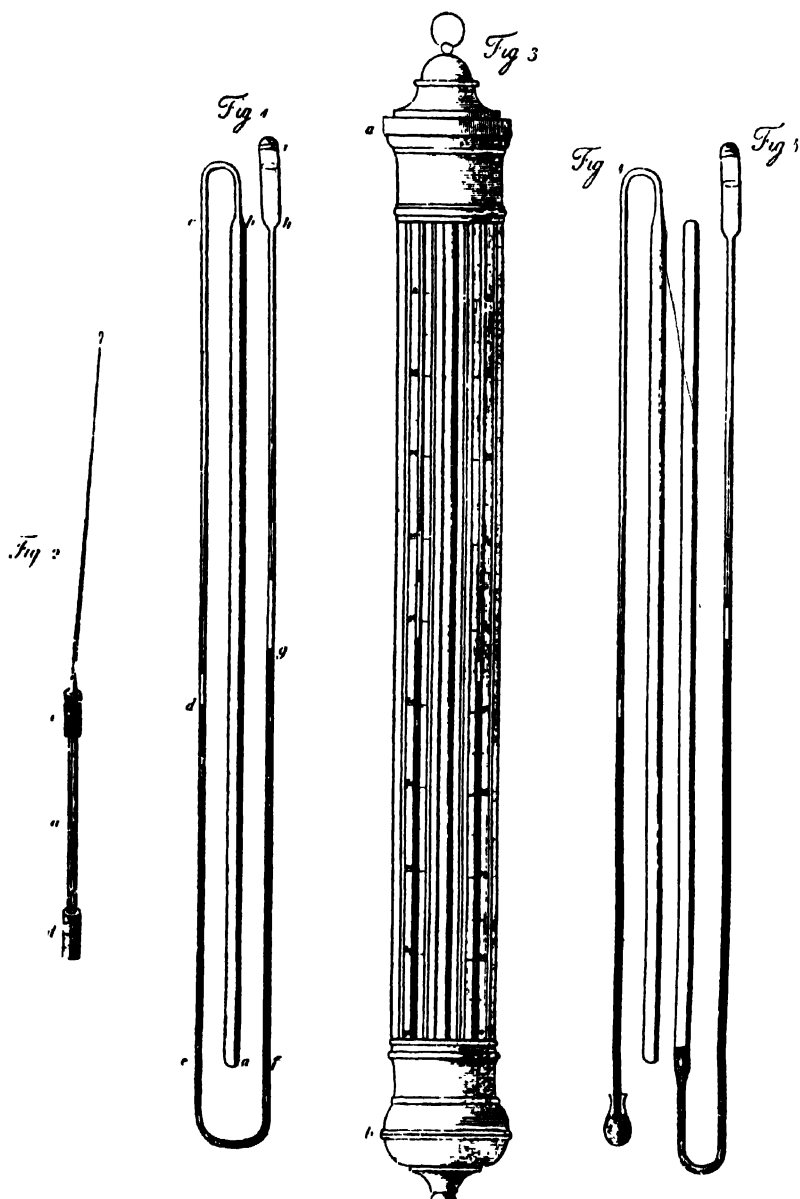
Illustr. 136.—Six's Combined Maximum and Minimum Thermometer

shows one of the indices on an enlarged scale: "*a* is a small glass tube, three-quarters of an inch long, hermetically sealed at each end, inclosing a piece of steel wire, nearly of the same length; at each end *cd* is fixed a short piece of a tube of black glass, of such a diameter as to pass freely up and down within the small tube of the thermometer. . . . From the upper end of the body of the index at *c* is drawn a spring of glass to the fineness of a hair, about five-fourths of an inch in length, which, being set a little oblique, presses lightly against the inner surface of the tube, and prevents the index from following the mercury when it descends." Fig. 3 shows the instrument mounted on its frame. "Towards evening," Six continues, "I usually visit my thermometer, and see at one view, by the index on the left side, the cold of the preceding night; and, by that on the right,
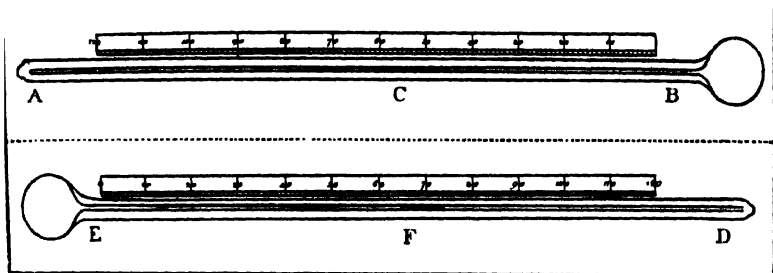
Illustr. 137.—John Rutherford's Maximum and Minimum Thermometer

the heat of the day. These I minute down, and then apply a small magnet to that part of the tube against which the indexes rest, and move each of them down to the surface of the mercury: thus, without heating, cooling, separating, or at all disturbing the mercury, or moving the instrument, may this thermometer, without a touch, be immediately rectified for another observation." Figs. 4 and 5 show the thermometer as constructed in two separate parts, to show the greatest heat and cold respectively. It will be understood that in Six's thermometer the alcohol is really the thermometric fluid; the chief function of the mercury is to move the indices.

A still simpler instrument, working on the same principle, but constructed in two separate parts, was described in 1790 by Daniel Rutherford, Professor of Medicine and Botany at Edinburgh University, and the first man to isolate nitrogen (*Trans. Roy. Soc. Edin.*, 1794, Vol. III, pp. 247 ff.). He, however, attributes the actual invention to one John Rutherford, M.D. In this instrument the minimum temperature was recorded by an ordinary alcohol thermometer AB. Its stem contained an index in the form of a small conical

piece of coloured glass or enamel C, with its point towards the bulb; this index was about half an inch long, and just fine enough to slide freely up and down the tube. Once the index was completely immersed in the spirit, it was not able easily to break through its surface. The thermometer was inverted so that the index slid to the top of the alcohol column, and it was then mounted in a horizontal position. If the temperature fell the alcohol contracted, and the index was drawn towards the bulb. Should the temperature thereafter rise and the alcohol expand, the index remained to mark the lowest reading registered. The maximum temperature was recorded by a mercurial thermometer DE; its index was a conical piece of ivory, F, with its base turned towards the bulb and initially resting on the mercury surface. As the mercury expanded with rise of temperature, it pushed the index before it; when the mercury contracted, it left the index behind to mark the highest temperature attained since the instrument was last set. The two thermometers were mounted horizontally on the same stand with their stems pointing opposite ways, so that the same movement served to reset them both.

A registering thermometer, constructed by Henry Cavendish and preserved at the Royal Institution (though now out of order), is described in George Wilson's *Life of Cavendish*, 1851 (p. 477); Wilson's account is reproduced in *The Scientific Papers of the Hon. Henry Cavendish*, Cambridge, 1921 (Vol. II, pp. 395 ff.). The front and back views of the instrument are shown in Illustr. 138. It consisted essentially of a glass tube containing alcohol, passing horizontally along the top of the instrument so as to be exposed to the atmosphere (though sheltered from rain), and communicating below with a U-tube. The expansion and contraction of the alcohol with changes of temperature moved a thread of mercury in the U-tube. The surface of the mercury in the left-hand, open limb of the tube carried an ivory float; to this was attached a silk thread which passed twice round a grooved wheel and hung down, supporting a small weight. To the axis of the wheel was affixed a light index traversing the graduated circle seen in the right-hand figure. On either side of the index was a friction needle, similar to those seen on the dials of domestic barometers; as the index began to move in either direction it pushed before it one of these needles, which thereafter remained stationary at the extreme limit reached by the index in that direction, when the latter began to recede. The instrument was reset by bringing the friction needles into contact with the index. The whole was enclosed in a case with glass faces and metal doors, and it stood about eighteen inches high. The projecting pegs seen in the left-hand figure may have
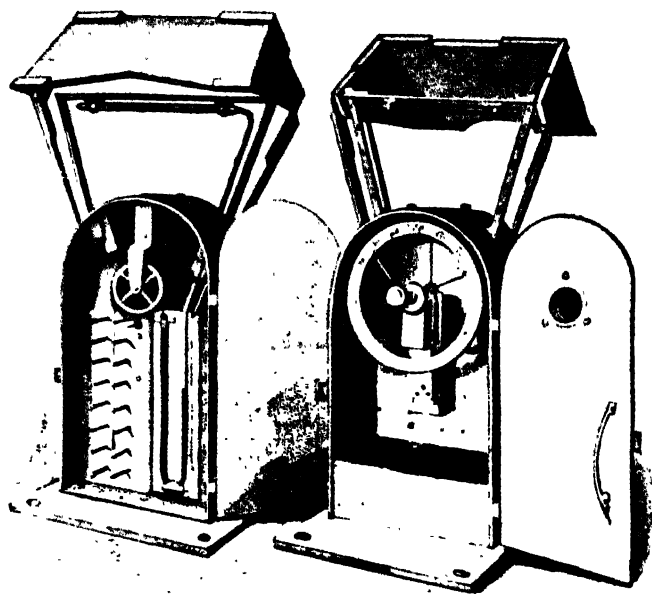
Cavendish's Registering Thermometer
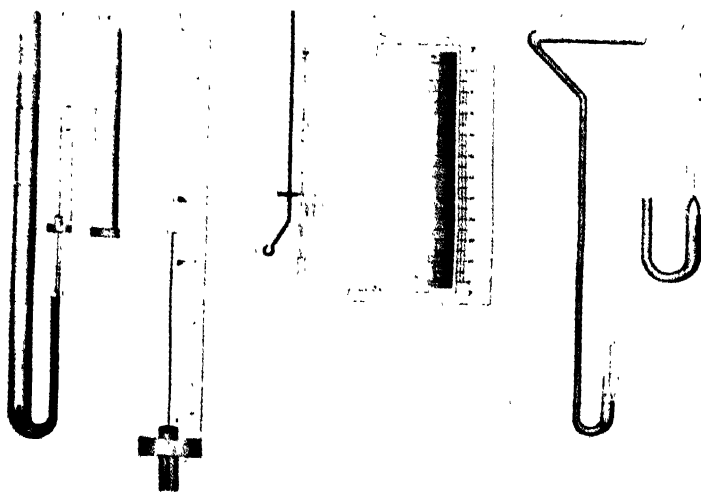
been intended to hold some desiccating substance to keep the interior dry.

A thermometer serving not only to show maximum and minimum temperatures but also to make a continuous graphical record of its own indications throughout a given period of time was devised about 1795 by Alexander Keith (*Trans. Roy. Soc. Edin.*, Vol. IV, 1798, pp. 203 ff.). His thermometer, shown in Illustr. 139, Fig. 1, consisted of a glass tube AB, about fourteen inches long and three-quarters of an inch in diameter, closed at the upper end, and terminating below in a tube BED of narrower bore, bent upwards and open at the top. The tube was filled with alcohol from A to B, and with mercury from B to E. Upon the mercury surface at E there was placed a small conical float of ivory or glass to which was attached a wire rising to H, where it was bent through a right angle and terminated in a short horizontal cross-piece. As the mercury rose or fell from its initial position with increase or decrease of temperature, this cross-piece raised or depressed one or other of two indices L, L; these consisted of slips of oiled silk sliding easily upon a fine gold wire fixed at its upper and lower ends by brass pins to a graduated scale, FG, which could be completely enclosed, together with the open end of the tube, by the cover II. The cross-piece H encircled the wire and pushed the upper or lower index before it, leaving it at the farthest point from the initial setting to show the highest or lowest reading recorded. The end of the tube, with the attached scale, wire, and indices, is shown on an enlarged scale in Fig. 2. When the thermometer was to act as a self-registering instrument, Keith recommended the following modifications: the thermometer was to be made on a larger scale, AB being about forty inches long; in place of the cross-piece at H, it was to be equipped with a short pencil Q (Fig. 3), which, by means of the weight R, was made to press lightly against a sheet of paper wound round a revolving drum MN. The paper was to be graduated by vertical lines into days of the month, and by horizontal lines into degrees Fahrenheit, and the drum was to be turned by clockwork so as to complete a revolution in one month. As the temperature rose and fell from day to day, the pencil would leave a sinuous trace on the paper, and the charts so obtained during twelve successive months, when collected together, would form a complete record of the temperatures at the place of observation for that year. In a subsequent paper (*ibid.*, pp. 209 ff.), Keith described how a siphon-barometer might be similarly equipped so as to show maxima and minima of atmospheric pressure (see Figs. 4 and 5).

B. ANEMOMETERS

During the seventeenth century, measurements of the strength of the wind had occasionally been made, after a fashion, with a primitive type of anemometer whose invention is generally attributed to Robert Hooke, though the instrument may have been in use before his time. It consisted essentially of a light plate, or shutter, of wood or metal, suspended at right angles to the direction of the wind by a rod which was free to turn about an axis passing through its upper extremity and parallel to the plane of the plate. As the plate was blown aside by the wind, the rod moved over a graduated scale, and so measured the strength of the wind in arbitrary units. The tendency of scientific instruments to fall into disuse, and to be subsequently re-invented, or reintroduced with improvements, is particularly marked in the history of anemometers. Thus the pendulum anemometer attributed to Hooke was re-invented in 1744 by Roger Pickering of Deptford (*Phil. Trans.*, Vol. XLIII, No. 473, p. 1), but with the addition of a catch which, when the plate had been deflected through any angle by the wind, prevented it from swinging back towards its position of rest. Thus the instrument registered the greatest *force* of the wind to which it was subjected during a given period of observation. It also comprised an index showing the *direction* of the wind at each instant. In a still more complicated form of the instrument, proposed by Dalberg in 1780 (see F. Rozier: *Observations sur la Physique*, XVII), the wind pressed upon a plate which was free to turn by a hinge about its lower edge, and to whose upper edge was fastened a cord passing over a pulley and carrying a weight which, as the plate was blown aside, was hoisted until its pull just balanced the pressure of the wind upon the plate. Dalberg's instrument also provided for measuring the *direction* of the wind, and its *inclination* to the horizontal. M. C. Hanov, in his *Philosophia Naturalis* (1765, Vol. II, pp. 142–3) describes his attempts to compare wind-strengths by finding which of a series of flags was just blown out horizontally, or by observing the deflection of a leaden globe suspended by a horse-hair. During the nineteenth century, spheres were substituted for flat plates in several further anemometers of the pendulum type.

In another important class of anemometers, the setting in motion of the movable parts by the wind was designed to call into play resisting forces which steadily grew with the motion until they sufficed to bring it to a standstill. Thus, in Christian Wolff's anemometer (*Aerometriae Elementa*, 1709), windmill sails turned a horizontal axle acting, through an endless screw, upon a toothed wheel having attached to it a radial arm, at the farther end of which a weight

was fixed. The force of the wind was measured by the angle (indi-
cated on a dial) through which the wheel turned before coming
to rest under the increasing moment of the weight. Leupold
(*Theatrum Machinarum generale*, 1724) and Leutmann (*Instrumenta
Meteorognosiae inservienta*, 1725) independently described an instru-
ment having the form of a paddle-wheel of six curved sails attached
to a vertical axle. The wind was directed on to the blades of the
paddles, and the revolution of the axle hoisted a weight whose
leverage upon the axle, however, was contrived (through the action
of a cam) to increase steadily until the motion ceased. In another
of the seven anemometers whose invention was claimed by Leupold,
the cord supporting the weight was wound on to an axle in the
form of a cone (somewhat like a fusee in a clock), the laps of cord
being wound from the thin end of the axle towards the thick end,
until a point was reached at which the moment of the weight acting
upon the axle was sufficient to arrest the motion. This device was
re-invented by Benjamin Martin in 1771 (*Philosophia Britannica*,
3rd ed.), and later applications of it were made in the nineteenth
century by Galton and Stokes.

Another type of anemometer employed during the eighteenth
century consisted of a piece of cardboard, or later of metal, attached
at right angles to a light rod which was inserted in a tube up which
it could be thrust against a spring. The board was presented to
the wind, whose strength was measured by the distance which the
rod was forced up the tube. In improved forms of this instrument
the rod was graduated and fitted with teeth which were caught
and held as the rod entered the tube (so that the maximum pressure
could be registered), and which, in some forms, worked a pinion-
wheel showing the pressure in conventional units on a dial. (See
P. Bouguer: *Traité du Navire*, 1746; Abbé Nollet: *L'Art des Expériences*,
1770, Vol. III; Rozier: *Observations sur la Physique*, XV). Wilcke,
in 1785, described an *anemobarometer* (*Magazin fur das Neuste aus der
Physik* (Gotha), Vol. III, pt. ii), in which the pressure upon a surface
exposed to the wind was transmitted by a lever, with mechanical
advantage, to a plunger, which was pressed against a leather bag
containing mercury. As the mercury was forced out of the bag,
it rose in a vertical glass tube issuing from it. The instrument was
made more sensitive by pouring in coloured alcohol above the
mercury, and noting its rise in a greatly constricted portion of the
tube.

Several of the anemometers constructed or proposed during the
eighteenth century were of a type consisting essentially of a U-tube
partly filled with some liquid and having one of its arms bent
outwards at the top through a right angle. The wind was allowed

to blow into the bent arm of the tube; its pressure forced down the surface of the liquid on that side and raised it on the other side, and the strength of the wind was measured by the difference in the levels of the two liquid surfaces. What seems to have been the earliest instrument of this type was described by Pierre Daniel Huet, an erudite French courtier and ecclesiastic, in a miscellany which he composed shortly before his death in 1721 and which appeared posthumously under the title *Huetiana; ou pensées diverses de M. Huet, Evesque d'Avranches* (Paris, 1722). In Section XX of this book, headed by the word *Anémométre* (the term seems to be due to Huet) we find a description of an instrument which he devised for "weighing the wind." Hubin, the instrument-maker, undertook to construct such an instrument, but he died before he could do so. Huet writes of his invention as follows: "It consists of a tin funnel ABC like the cowl of a monk. This funnel curves round, contracting as it curves, to C; here a tube begins which descends to D, and thence it curves through DIE and rises to K where it ends. The tube is filled with mercury from CDE to F. Lye-water is poured in above F, as far as G, and its rise and fall are observed with the aid of little dots which are marked on the tube from F to G. The wind entering by the funnel AB strikes the mercury surface at C and presses it more or less according to its force. The mercury so pressed descends in proportion to the pressure upon it, and, descending on the side next the funnel, it rises in the other branch of the instrument above F, and raises the lye-water which it supports, whose rise is observed and measured against the dots marked on the tube. And because the instrument cannot act if the funnel is not turned towards the quarter from which the wind is blowing, the vane M must be affixed, supported by the iron rod MHI. This rod forms a ring at the point I which encircles and firmly grips the tube. Below the ring, the iron rod enters a collar L, mounted on the pedestal LNO, in which it turns to right and left, according as the wind turns the vane, and

Illustr. 140.—Huet's Anemometer

so it turns the whole instrument, and keeps the funnel always directed towards the wind" (Huet: *op. cit.*, pp. 55-8). The best known instrument of this type, however, was that of which a detailed and illustrated account was given in 1775 by James Lind in the *Philosophical Transactions* (Vol. LXV, p. 353). Lind used water as the liquid in his instrument, and he constructed tables claiming to give the wind-pressures (in pounds to the square foot) corresponding to given differences in level of the water in the two arms of the tube. With a view to registering the greatest strength of the wind experienced during a period of observation, Lind recommended cutting the leeward limb of the U-tube off short at the normal level of the water therein, and subsequently measuring the quantity of water which overflowed from that limb during the period considered.

Lind's "wind-gage" consisted of two glass tubes AD, CB, each about six inches in length, and connected to each other by means of the bent glass tube *ab* (Illustr. 141). The limb AD was surmounted by a short brass tube with its mouth F turned outwards; this was connected to the cover G of the other limb by a slip of brass *cd* which supported the upright scale HI. The whole instrument was free to turn about the spindle KL so that F should face the wind. To measure the force of the wind the tubes were half filled with water, and the position of the scale was adjusted until the zero at its centre coincided with the common level of the two water surfaces. The instrument being then presented to the wind, the distances through which the water level was depressed in one limb and elevated in the other were measured on the scale. The sum of these distances represented the height of a column of water which the force of the wind was capable of sustaining at the time of the observation.

All the earlier anemometers were thus essentially *pressure-gauges*, perhaps because, in practice, and especially to sailors, the *pressure* of the wind is its most important measurable property. Several eighteenth-century anemometers, however, were designed to measure the *velocity* of the wind. Rough and ready estimates of wind-velocity were obtained in the seventeenth and early eighteenth centuries by observing the speed at which light bodies, such as feathers, were swept along on the breeze. Observations of this kind had already been made by Mariotte about 1680, when Derham, in 1708, described some of the attempts which he had made upon the problem in connection with his researches on the velocity of sound (*Phil. Trans.*, 1708, p. 2). From observations of the motion of down in the wind, he was led to conclude that even the strongest winds do not exceed velocities of about fifty or sixty miles an hour. It was

later recognized that a floating body does not attain the velocity of the current which bears it along, and, moreover, that the onrush of the wind is considerably checked near the Earth's surface, so that, in any case, such observations could not give any information of meteorological value concerning the rate at which quantities of air are transferred from one region to another. Alexander Brice, experimenting half a century later on the same lines as Derham, was struck by the great irregularities, both in speed and in direction, of the motions of feathers released in the wind, which made accurate calculations impossible. He preferred to base his estimates on the measured velocities of cloud-shadows. He chose days of bright sunshine broken by high clouds moving rapidly across the sky, and he obtained very consistent results for the distances traversed (in fifteen seconds by the clock) by the edges of successive cloud-shadows (*Phil. Trans.*, 1766, p. 224). The comparison of wind-velocities by observations of the motion of a feathered cork disc along a wire, was suggested by Bouvet (*Hist. de l'Acad. Roy. des Sciences*, Paris, 1733). Instruments for measuring the velocity of wind, constructed during the eighteenth century, were frequently very complicated in design, but generally disappointing in performance owing to the cumbrousness of the movable parts, and the friction of the numerous bearings. An instrument of this type, devised by Dinglinger about 1720 (see Leupold: *Theatrum Machinarum generale*, 1724), was imitated by d'Ons-en-Bray (*Hist. de l'Acad. Roy. des Sciences*, Paris, 1734), whose apparatus had a vane to show the direction, and a moving wheel to indicate the velocity, of the wind, together with a clockwork appliance for automatic registration, though the instrument seems to have been of little value. Lomonosow, about 1750, described an anemometer in which the wind was to work a small paddlewheel (suitably orientated by means of a vane) whose motion was to be transmitted through gearing to a room below, where the revolution of a wheel would give some idea of the velocity of the wind (*Novi Commentarii Academiae Petropolitanae*, II). A more practically useful device for measuring the velocity of wind, and of fluid currents in general, was that described by R. Woltmann in his *Theorie und Gebrauch des hydrometrischen Flügels* (Hamburg, 1790). The relation of the velocity of a fluid to its pressure, and power of doing work, was very thoroughly studied, with the aid of ingenious experiments, by John Smeaton, about the middle of the eighteenth century. In particular, he investigated the weight-lifting power of a windmill subjected to an artificial wind of known velocity produced by whirling it round on the end of a cross-bar turning about a vertical axle (*Phil. Trans.*, 1759, p. 100). Similar observations were later made on Bouguer anemometers. The familiar
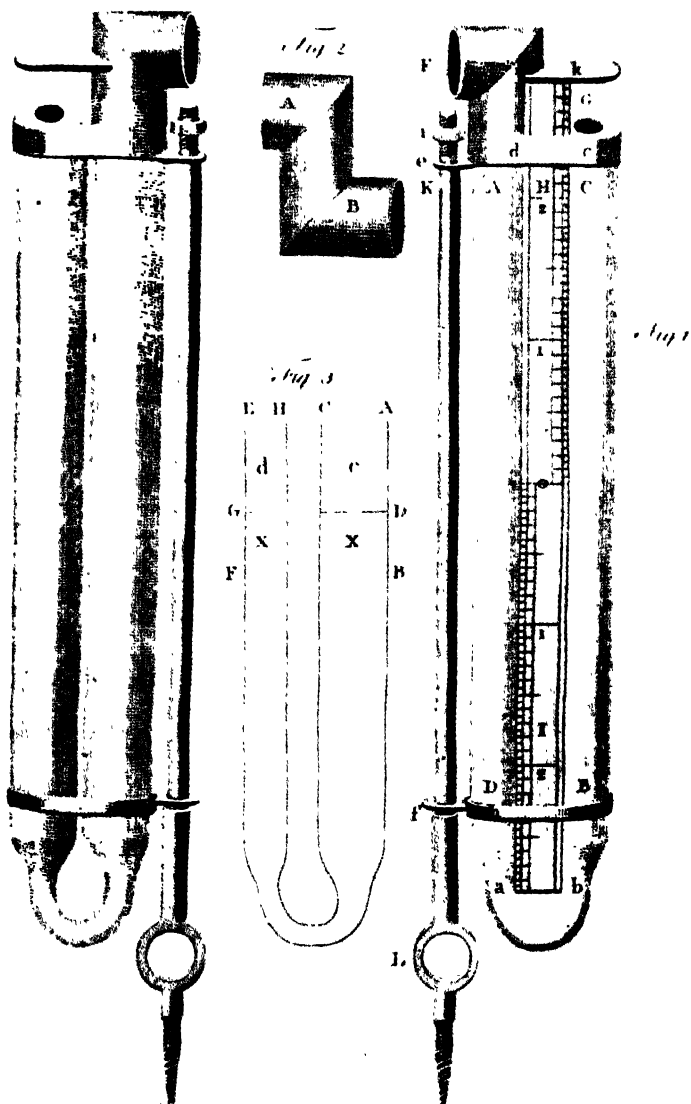
Lind's "Wind-Gage"
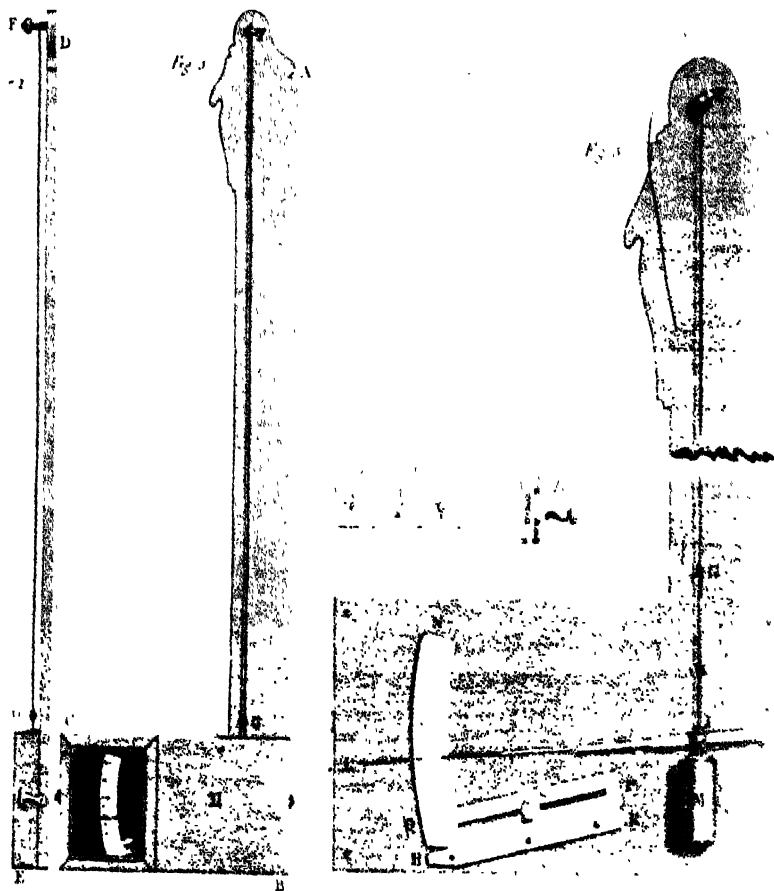
Smeaton's Hygrometer

type of hemispherical cup anemometer was brought into use about the middle of the nineteenth century, to which period also belong the various devices for measuring the velocity of the wind by its *physical* effects, such as cooling, evaporation, suction, or the production of musical sounds.

(See J. K. Laughton: *Historical Sketch of Anemometry and Anemometers* in *Quarterly Journal of the Meteorological Society*, 1882, Vol. VIII, No. 43.)

C. HYGROMETERS

During the eighteenth century, many physicists exercised their ingenuity on the invention of improved forms of hygroscopes or hygrometers, for indicating, as accurately as possible, the humidity of the atmosphere. The principles underlying the eighteenth-century forms of these instruments had nearly all been employed already in one or other of the hygroscopes of the seventeenth century. And throughout the period here considered, the means available for estimating humidity never reached such a level of precision and uniformity as was meanwhile attained in the measurement of pressure and temperature. The scientific literature of the period contains numerous descriptions and illustrations of hygrometers (some actually constructed, others only projected), with various opinions as to their relative merits; but only a few typical or scientifically important types can here be noticed. The discussion of problems connected with the construction of these instruments resulted in the formulation of some of the principles of *hygrometry*, considered as a special branch of applied physics.

Several eighteenth-century hygrometers were developments of an instrument originally described by Robert Hooke (in his *Micrographia* and elsewhere). This showed changes of moisture by the twisting and untwisting of the beard of a wild oat (or of some other such fibre), to whose free end was attached a pointer arranged to move over a graduated scale. Dalibard's hygrometer (*c.* 1744) worked on the same principle; he supported, in a perforated brass tube, a length of catgut, one end of which was attached to the bottom of the tube, while the other end was free to turn a pointer over a graduated circle as the twist of the gut varied with the humidity (L. Cotte's *Mémoires sur la Météorologie*, 1788, I, p. 231).

Most organic substances vary in bulk according to the humidity of the atmosphere; such substances may therefore be used as hygrometers if means are adopted to measure the changes (suitably magnified) in their linear dimensions, or in the volumes of receptacles made from them.

The simplest, and not the least satisfactory, of early hygrometers working on this principle, consisted of a cord fixed at one end to a nail in a wall, and kept taut by a weight attached to the other end. Variations in the humidity were shown by the rise and fall of the weight. In a rather more refined form of this apparatus, the stretched cord passed over a pulley-wheel, which was turned, by expansions and contractions of the cord, through small angles which were indicated by a pointer attached to the wheel and traversing a graduated scale (see, for example, C. Wolff's *Aerometriae Elementa*, 1709, and John Dalton's *Meteorological Observations and Essays*, 1793). Increased sensitiveness in the indications of such "weather-cords" was sought by William Arderon (*Phil. Trans.*, 1746, p. 95). He fixed his cord at both ends, and suspended a weight from the middle by means of a silk thread. As the cord contracted and expanded the thread rose and fell and worked a pivoted pointer, one end of which was attached to the middle of the thread, while the other end traversed a scale where the oscillations of the system were greatly magnified. In an improved form of this apparatus the pointer was worked by a rack and pinion. Those who worked with such "weather-cords," however, soon observed their tendency to stretch with lapse of time. John Smeaton accordingly sought to standardize his cord hygrometers from time to time by measuring their lengths when they were perfectly dry, and again when they were completely saturated with moisture; and he divided the difference of the lengths into one hundred equal divisions (Illustr. 142). He also sought to make his cords more susceptible to moisture in the air by giving them a preliminary boiling in brine (*Phil. Trans.*, 1771, p. 198). The most refined by far of hygrometers of this class which the eighteenth century produced were those of Horace Bénédict de Saussure (1740–99). He utilized hygroscopic variations in the length of a human hair, employing a number of ingenious devices for making these variations appreciable; and he did much to develop hygrometry as a science.

De Saussure was primarily a geologist, famous for his exploration of the Alps and for his ascents of Mont Blanc and of Monte Rosa. An account of his important researches in hygrometry is to be found in his *Essais sur l'Hygrométrie*, Neuchâtel, 1783 (of which there is an annotated German translation in Ostwald's *Klassiker*, Nos. 115 and 119). He began these researches many years before the publication of his *Essais*; but his progress was delayed by the difficulties which he encountered, by the many subsidiary investigations into which he was led, and by his frequent absences on expeditions among the Alps. The work consists of four Essays. The first of these contains a detailed description of the two forms of hair-hygrometer

invented by De Saussure, with instructions for their manufacture. The second Essay treats of the general principles of hygrometry as a science. The third and fourth Essays deal respectively with evaporation, and with the application, to the outstanding problems of meteorology, of the results set forth in the preceding portions of the book. Cuvier regarded De Saussure's *Essays* as one of the greatest contributions made to science in the course of the eighteenth century.

The idea of utilizing the expansions and contractions of a hair as an index of humidity occurred to De Saussure in 1775, though it was some years before he hit on a process for rendering hairs sufficiently sensitive and durable for his purpose. A taut hair lengthens when wetted and contracts when dried, over a range which may amount to about one-fortieth of its length. The problem was to make this variation sensible in an instrument of convenient size. De Saussure devised two rather different forms of hair-hygrometers; one of them is shown in Illustr. 143. The lower end of the hair, *ab*, is held by the screw-clamp *b*; the other end is clamped at *a* to a strip of foil wound on the horizontal cylinder *d* which, as it revolves, carries a pointer round a graduated dial. A counterpoise *g* suspended by a silk thread, which is wound on the cylinder the
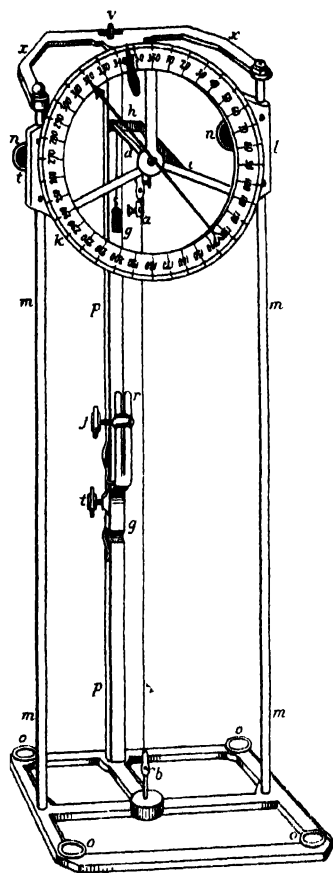
Illustr. 143.—De Saussure's Hair-Hygrometer

opposite way to the foil, keeps the hair taut. Slight changes in the length of the hair cause appreciable changes in the reading on the dial. De Saussure found this form of hair-hygrometer too fragile for safe transport on his expeditions. He accordingly designed a second form of the instrument which was more portable, though less sensitive, than the first. The pointer was here counterpoised, on

the pivot, by a circular sector of metal along whose rim ran two grooves; the hair lay in one of these and was gripped at its lower extremity to a cross-piece turning with the needle; in the other groove lay the thread supporting the counterpoise; it was fixed at its upper end to the other end of the cross-piece. The descent of the counterpoise raised the needle until the hair was just pulled taut, and the needle travelled over a scale as the hair fluctuated in length. Pocket instruments of this type could be constructed. De Saussure found it best to use fine, soft hairs, preferably blond, cut from the head of a living, healthy person. He freed them from their natural grease, and made them absorbent of moisture, by boiling them in a soda solution of appropriate strength. When the instrument had been put together, it was necessary to mark the positions of the needle when the hair was exposed respectively to conditions of complete saturation and complete dryness. The point of greatest humidity was fixed by placing the instrument in a bell-jar which stood in a dish of water, and whose interior surface was kept thoroughly wet until the hair ceased to lengthen, when the position of the needle was marked. The temperature of the water seemed to be a matter of indifference. Hairs which continued stretching indefinitely or showed anomalous contractions under these circumstances, were to be rejected. In fixing the point of greatest dryness, the hygrometer was confined in an air-tight bell-jar with a chemical drying agent, until the needle had become stationary. To make sure that the desiccation was complete, the whole was then exposed to the heat of the Sun or of a fire; if any moisture was left, the hair would expand and then gradually contract, but if the enclosure was quite dry, only the slow thermal expansion of the hair would be observed, and the position of the pointer at a mean temperature under these conditions could be taken as the dry-point. In graduating the instrument, the interval between the dry-point and the wet-point was divided into a hundred degrees, so that the readings increased with increasing humidity. Or else the dial was graduated into arbitrary units, which could be converted into degrees of humidity by means of a conversion-factor which would be different for each hair fitted to the instrument. Once a hygrometer had been graduated between such absolutely determined fixed points, others could be graduated by comparison with it.

Illustr. 144.—De Saussure's Pocket Hygrometer

In his Essay on the theoretical principles of hygrometry (which

he conceives as "the art of measuring the absolute quantity of water suspended in the air"), De Saussure distinguishes three classes of methods for determining humidity, which respectively utilize (i) observations of changes in the weight, dimensions, or shape, of a hygroscopic body; (ii) observations of the capacity of the air for taking up water; and (iii) observations of the quantity of water condensing from the air under given conditions on a cold surface, or of the degree of cold necessary to start such condensation. His theory of the methods grouped under (i) is that different substances have different affinities for water, these affinities increasing with the dryness of the substances; and the moisture available to a system of bodies is so divided among them that their competing affinities are brought into equilibrium. Hence the complete desiccation of a system of hygroscopic bodies by means of a drying agent is, he holds, never possible. It is the constant relation subsisting between such affinities when in equilibrium which makes hygrometry possible: "A cord hygrometer shows only the state of the cord which moves its needle, but as there is a definite relation between the attractive force of the cord and that of the air, it follows that the state of the cord necessarily depends upon that of the air in which it is immersed, and that therefore one can deduce with certainty the condition of the air from that of the cord." Later, and especially after the work of Dalton on the properties of mixtures of different gases, it was recognized that it is not the affinity of air-particles for moisture which prevents all the moisture in an enclosure from being absorbed by the other hygroscopic bodies present, but rather the existence of a free space, having a definite content of water-vapour existing in an equilibrium (whose conditions depend upon the temperature) with the water absorbed by these bodies. De Saussure's explanation of methods (ii) is that the air is capable of saturation, so that, other things being equal, the actual humidity of an enclosed quantity of air is inversely related to the quantity of water necessary to saturate it. But the weakness of such methods, he thinks, is that evaporation into the enclosure tends to continue even after saturation has been reached, the excess of moisture being deposited somewhere on the walls of the containing vessel. Among the methods numbered (iii) he classes the weighing of water condensing in a given time upon the surface of an ice-filled vessel, as practised by the *Accademia del Cimento*, and also C. Le Roy's procedure (*vide infra*) of noting the stage at which dew forms on a vessel of water gradually chilled with ice. Such methods, however, cannot be used at temperatures below freezing-point, or when the air is very dry; and the temperature at which dew is deposited upon a surface is affected to some extent by the conditions of the surface in question.

De Saussure next enumerates what he considers to be the ideal characteristics of a hygrometer: (i) it must be sensitive to changes of humidity; (ii) it must be prompt to respond to such changes; (iii) it must be self-consistent, always showing the same degree for the same state of the air; (iv) different hygrometers of the same type must show the same readings under the same conditions; (v) the hygrometer must be affected only by the *water*-vapour present; (vi) its indications must be proportional to the concentration of this vapour. De Saussure considered that hair-hygrometers in the main satisfied these requirements. But actually they show only relative humidity; and he himself found some experimental evidence suggesting that they might be slightly affected by the vapours given off from certain volatile oils.

The influence upon the reading of the hair-hygrometer of the concentration of the water in the air, as well as of the temperature, density, and agitation, of the air, are next considered. Heat produces a thermal expansion of the hygroscopic hair. This may be studied while the instrument is in a completely dry atmosphere, and temperature-corrections may then be deduced for application to the readings of the instrument when in normal use. Besides such purely *thermal* expansions and contractions, De Saussure investigated the more complicated problem of the behaviour of the hygrometer when exposed to changes of temperature in the presence of water-vapour. His procedure was to isolate a hygrometer and a thermometer in a gas-tight enclosure, and to observe how the indications of the hygrometer varied with changes of temperature. He repeated this experiment with different amounts of moisture in the enclosure, and he summarized his results in a table showing the variations of the hygrometer-reading corresponding to a change of 1° in temperature at each hygrometer-reading between 25° and 100°. This table was intended for correcting hygrometer-readings for differences of temperature, so as to make them comparable. It was converted into a table showing the changes of temperature necessary to change the indication of the hygrometer by 1°, at each degree of its scale, and hence enabling the number of degrees of heat to be calculated through which the thermometer would have to fall in order to bring the air to saturation-point. De Saussure studied next the relation of the *quantity* of water present in a given enclosure, at a given temperature, to the indications of a hygrometer confined therein. His procedure was to find what quantity of water was necessary to saturate such an enclosure, and subsequently to introduce definite fractions of this quantity, and to observe meanwhile the behaviour of the hygrometer. From an experiment in which he allowed water to evaporate to saturation into a previously dried

receiver, from a vessel which was weighed before and after the evaporation, he calculated (dividing the loss of weight of the vessel by the volume of the receiver) that 11 grains of water were necessary to saturate 1 cubic foot at about 15° (on a thermometer with fixed points at 0° and 80°). In studying the effects of *fractions* of this concentration, De Saussure used a large ellipsoidal receiver of about 4·25 cubic feet capacity, containing a barometer as well as a hygrometer and thermometer. He suspended a moistened cloth in the previously desiccated receiver, until the barometer had risen by a definite amount in consequence of the addition of water-vapour; he observed the change in the hygrometer-reading and found the loss of weight of the cloth. He repeated the experiment over the next portion of the hygrometer scale, and so on, six times in all, correcting for slight changes of temperature in the course of the experiments, and correlating the reading of the instrument with the actual concentration of the water in the receiver. Experiments on somewhat similar lines had already been made by J. H. Lambert (*Royal Academy of Berlin*, 1769), to whom the name of the science of hygrometry appears to be due. He had, however, at his disposal only a primitive gut-hygrometer with which to trace the progress of evaporation, and in seeking to measure the quantity of water necessary to saturate an enclosed space, he seems to have neglected the tendency of the vapour to condense on the walls of the containing vessel as soon as, or before, saturation was reached—a danger of which De Saussure was better aware.

When De Saussure placed a hygrometer in the receiver of an air-pump, and partially exhausted the air, the reading of the instrument fell towards *dry*. The pointer subsequently returned towards its starting-point, but stopped short of it. De Saussure explains that dilation of the vapour reduces its effect upon hygroscopic substances; but probably temperature-effects, accompanying the exhaustion, also played a part in what he observed. He projected, and partly constructed, tables showing the inter-relations of the degree of saturation of air, the quantity of vapour per cubic foot, the temperature, and the pressure, of the air. De Saussure knew that air possessed more drying power when in motion than when at rest, but he was uncertain whether moving air actually required more moisture to saturate it, bulk for bulk, than stagnant air. He noticed that when, on a calm day, a little breeze sprang up, it would send the hygrometer towards *dry*, though presumably all the air in the neighbourhood was equally charged with moisture. He tried the experiment of shutting up a clockwork windmill in a bell-jar with a hygrometer; when the mill was set going, there was a displacement of the pointer towards *dry*, but this seemed to be accounted for by the measured

rise of temperature in the enclosure (attributed to the friction of the mill), which would increase the "dissolving power" of the air. De Saussure concluded that, very likely, the breezes which dried the hygrometer really did so by introducing intrinsically drier air. Contrary to the current opinion that electrification favoured evaporation, De Saussure found that giving his hygrometer a strong electric
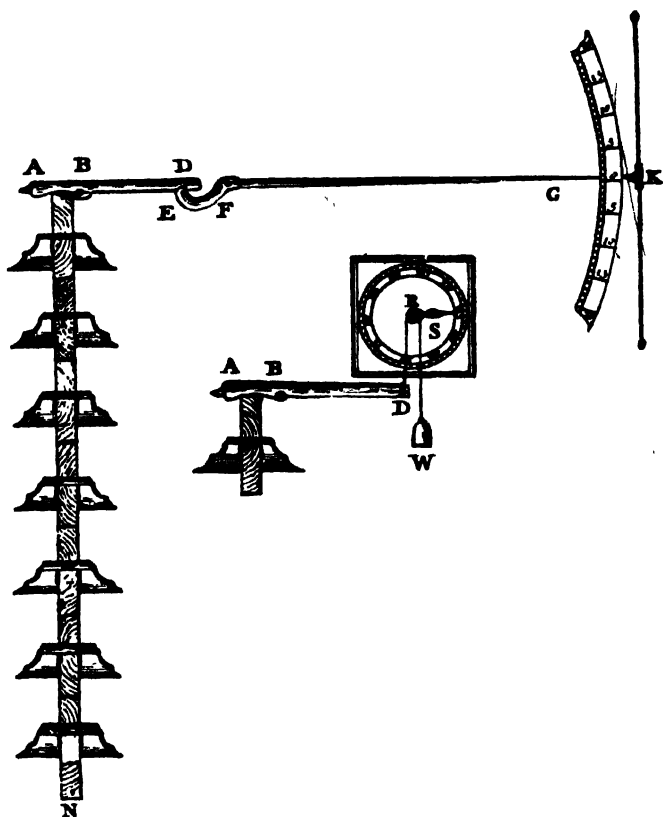
Illustr. 145.—Arderon's Board Hygrometer

charge did not appear to affect its indications. The effect of aqueous vapour upon the instrument seemed to be the same in atmospheres of "fixed air" (carbon dioxide) or of "inflammable air" (hydrogen) as in ordinary air.

The third Essay, discussing the phenomena of aqueous evaporation and condensation, in the light of then current theories, attributes such evaporation to the union of elementary fire with particles of water, giving rise to an elastic vapour which dissolves chemically

in the air. The chief point in the concluding Essay, on the meteorological applications of hygrometry, is De Saussure's attempt to disprove De Luc's hypothesis that the barometer falls when the atmospheric air becomes lighter, bulk for bulk, in consequence of the admixture of water-vapour. De Saussure's experimental data suggested that the addition of vapour, even to saturation-point, could not make sufficient difference to the density of the atmosphere to account completely for observed fluctuations in the height of the barometer.

Fig. 2.

The alterations of breadth in boards, consequent upon their absorbing moisture, had already been utilized for hygroscopic purposes in the seventeenth century. This phenomenon served as the basis of another of Arderon's hygrometers (*Phil. Trans.*, 1746, p. 184). He sawed off seven strips of deal, each measuring 10 inches by 1 inch by 1 inch, the length of each lying across the grain. He glued them end to end to form a rod one end of which was fixed at N, while the hygroscopic oscillations of the other end were conveniently magnified by means of a lever ABD, the end of which either worked a

Illustr. 146.—De Luc's Whalebone Hygrometer

pointer FG in the form of another lever, directly, or else moved a thread passing over a cylinder R, to which a pointer S was attached. An attempt was made, with the aid of a thermometer placed near by, to allow for the effect of changes of temperature upon the length of the rod. (Illustr. 145, p. 332.)

A hygrometer invented by De Luc, which employed a transversely

cut strip of whalebone, belongs to this class, though its mode of construction seems to have been suggested by that of De Saussure's hair-hygrometer. The instrument is described by its inventor as follows: "The slip of whalebone is represented by a, b; and at its end a is seen a sort of pincers, made only of a flattened bent wire, tapering in the part that holds the slip, and pressed by a sliding ring. The end b is fixed to a movable bar c, which is moved by a screw for adjusting at first the index. The end a of the slip is hooked to a thin brass wire, to the other end of which is also hooked a very thin silver gilt lamina, that has at that end pincers similar to those of the slip, and which is fixed by the other end to the axis by a pin in a proper hole. The spring d, by which the slip is stretched, is made of silver gilt wire; it acts on the slip as a weight of about twelve grains, and with this advantage over a weight (besides the avoiding some other inconveniencies of this) that, in proportion as the slip is weakened, in its lengthening by the penetration of moisture, the spring, by unbending at the same time, loses a part of its power. The axis has very small pivots, the shoulders of which are prevented from coming against the frame, by their ends being confined, though freely, between the flat bearing of the heads of two screws, the front one of which is seen near f. The section of that axis . . . is represented in Fig. 2; the slip acts on the diameter a, a, and the spring on the smaller diameter b, b." (*Phil. Trans.*, 1791, p. 389.)

Among the eighteenth-century hygrometers which utilized the changes of volume of receptacles made of organic materials, De Luc's was again the most noteworthy (*Phil. Trans.*, 1773, p. 404). The principle of this instrument, however, had been anticipated by Amontons in the seventeenth century, and there were several modifications of it subsequent to De Luc. The instrument consisted essentially of a small tube of ivory, about $2 \cdot 5$ inches in length, and $2 \cdot 5$ lines in diameter, drilled in the direction of the fibres. One end of the tube was closed, and to the other end was fixed, by means of a brass collar and cement, a glass thermometer-tube about 14 inches long. The ivory tube, and the lower portion of the glass one, were filled with mercury; and the principle upon which the instrument worked was that hygroscopic changes in the volume of the cylinder caused the mercury to rise and fall in the glass tube against a graduated scale, whose zero was fixed initially by immersing the ivory tube in a mixture of ice and water, and marking the lowest level reached by the mercury. By means of a thermometer mounted upon the deal base of the instrument, corrections for changes of temperature were applied to its indications of humidity. For the ivory cylinder there was later substituted a goose-quill, as in the hygrometers sent out by the *Mannheimer Gesellschaft*.

De Luc's hygrometers were described in the course of a series of papers on hygrometry contributed by him to the *Philosophical Transactions*. In the first of these (1773, p. 404), De Luc lays down the essential requisites of a good hygrometer, namely, (i) a fixed point from which to measure humidity; (ii) degrees comparable between different hygrometers; (iii) equal differences in the pointer-reading to be produced by equal differences of humidity. Under (i), extreme humidity seemed to offer the only chance of establishing a fixed point, and De Luc therefore proposed to soak the hygroscopic body in water of a definite, reproducible temperature, e.g., that of melting ice. The hygroscopic substance must therefore be such as can be *affected*, but not radically *altered*, by immersion in water, and De Luc was thus led to choose ivory in the first instance. In a second paper on hygrometry (*Phil. Trans.*, 1791, p. 1), De Luc describes the results of nearly twenty years' further experiments on the best methods of ascertaining the fixed points of hygrometers, and of arriving at an absolute hygrometric scale. He conceived the absorption of water into the pores of a hygroscopic body as analogous to its rise in fine capillary tubes. "When the quantity of liquid common to capillary tubes is not sufficient for them to receive their respective maximum, they share it between them, and the equilibrium takes place, when there is, in each of them, the same ratio between its specific capillary power and the weight of the raised column. In the same manner, when the quantity of water disseminated in a space is not sufficient for several hygroscopic substances to receive the maximum of water which they can contain in their pores, they share it amongst them; and the equilibrium is produced, when there is in each of them the same ratio, between its specific capillary power, and the resistance of their pores to be more dilated." De Luc determined the dry-points of his hygrometers by confining them in an enclosed vessel with a desiccant. He tried using potash, and several other alkaline substances, for this purpose, but finally he adopted quicklime, on learning, from James Watt, about the results obtained by Joseph Black with that substance. He constructed a special apparatus for hanging the hygrometers in a cage surrounded by quicklime fresh from the kiln, with a glass plate through which their dials could be viewed. A sensibly constant and permanent degree of dryness was obtained when several different forms of this apparatus were used. De Luc obtained the point of maximum humidity on a hygrometer by immersing the instrument in water, since he found that the deposition of water on the walls of an enclosure, or the formation of dew in the open, was no sure indication, especially at the higher temperatures, that the air had reached the limit of humidity. The temperature of the water appeared to have

no effect upon the purely *hygroscopic* alterations in the sensitive parts of the instrument. De Luc tabulates, comparatively, the results of experiments which he carried out on the hygroscopic changes of *threads* and *slips*, respectively, of whalebone, of quill, and of deal, between limits of extreme dryness and humidity. (By *threads*, he meant fibres, and by *slips*, thin strips cut across the fibres.) His tables show also the way in which shavings of the above-mentioned substances increased in weight concomitantly with the expansions of the slips and the threads. The balance-beams from which the shavings were suspended, and the slips and threads themselves (whose changes of length worked pointers), were confined in an air-tight, glass-fronted vessel into which such doses of moisture were periodically introduced as to make the whalebone hygrometer move about five degrees at a time. The threads differed in behaviour from the slips of like substance, as well as from the threads of other substances, their changes of length showing anomalous reversals; but the different slips showed much better agreement, and were therefore, De Luc supposed, better indicators of the actual humidity. He found that the movement of the slips "was more proportional, than that of threads, to the correspondent changes in weight of every hygroscopic substance of the elastic kind." But he "could not find any solid reason to consider the changes in weight of a substance, as being more proportional, than its changes in dimensions, to the correspondent changes of moisture in the medium," and he admits elsewhere in his paper that "the steadiest hygroscopic substances are subject to anomalies" which "will probably prevent our ever having in the hygrometer an instrument nearly so exact as the thermometer." A third paper on hygrometry contributed by De Luc to the *Philosophical Transactions* (1791, p. 389), contains the description of the whalebone hygrometer already given; it extends the tables

Illustr. 147.—De Luc's Instrument for securing a fixed degree of dryness in Hygrometers

of comparative hygroscopic expansions of threads and slips of divers substances; but it is mainly a criticism of the work of De Saussure, on the ground that the hair which he employed was of the unreliable class of *threads*, instead of being one of the steady-going *slips* which De Luc favoured. Referring to De Saussure's attempts to correlate the expansion of a hair with the increasing concentration of water-vapour in the surrounding air, De Luc challenges the other Genevan physicist's assumption that the amount of moisture, in an enclosure initially dry, increased proportionally to the amount of water which had evaporated into it. Actually, De Luc holds, on the strength of experimental results of his own, that "Moisture, or the quantity of vapour spread in the medium itself, does not increase in an inclosed space in proportion to the quantity of water evaporated in it; because of an increasing, but undetermined, part of that water being deposited on the sides of the vessel; and that, consequently, Mr. de Saussure's experiments could not afford the determination of a real hygroscopic-scale. Secondly, that the circumstance considered by him as a sure sign of extreme moisture existing in the inclosed medium, namely, the maximum of evaporation in the space, has only that effect when the temperature is very little above 32°; but that, by successive increases of heat from that point, moisture recedes farther and farther from its extreme; or from the point where no more vapour can be introduced in the medium without an immediate precipitation; though at the same time, there are successive increases in the quantity of vapour, and thereby a constant maximum of evaporation correspondent with the actual temperature."

Some kinds of hygrometers utilized the variable weights of suitably absorbent materials as a criterion of the moisture of the atmosphere. For this purpose a sponge was frequently employed; it could be hung from one arm of a balance, and variations in the amount of moisture which it absorbed from the atmosphere would be represented by variations in the weight required to counterpoise it. In some instruments of this sort, the sponge showed changes of humidity by its rise and fall. Desaguliers describes such a hygrometer, devised by Hales and himself, in which the suspending threads of the sponge and of the counterpoise were wound in opposite ways upon a conical axle or fusee, so that, as the counterpoise ascended, its moment about the axle would steadily increase until the motion ceased. Desaguliers writes: "P*nup*C is a Piece of Lignum Vitae cylindrick at C*n*P, but a truncated Cone from C*n* to *p*, and screw'd like the Fuzee of a Watch, but not near so taper. The Length of the Instrument is about a Foot, the cylindrick Part an Inch in Diameter, and half an Inch long. The large Part of the Screw about ⅔ of an Inch, and the small Part half an Inch. There are fine

Steel Pivots at each End, bearing on two fine conic Holes in Brasses in the Frame that carries the Instrument, that it may turn easily. A Spunge S hangs by a Silk from the Cylinder of the Instrument, so as to turn the Instrument by its rising or falling. A Weight W hanging from another Silk *u* coiled upon the Screw Cp, keeps the Spunge *in aequilibrio*. Now when the Spunge becomes heavier by imbibing Moisture from the Air, it runs down, and draws up W;

but as W comes up, its String must advance towards C*n*, where hanging farther from its Center, its Power will be so increas'd, that it will keep the Spunge *in aequilibrio*, tho' its Weight be increas'd: but as the Weight rises, it will show on the Scale DD how much the Spunge is heavier, and consequently the Air moister" (*A Course of Experimental Philosophy*, London, 3rd ed., 1763, Vol. II, pp. 299, 300). In other forms the sponge was counterpoised by means of lead shot strung on a thread the lower portion of which rested on a table, as in an instrument described in 1746 by Arderon (*Phil. Trans.*, 1746, p. 95). The descent of the sponge raised some of the shot from the table and so increased the effective weight of the counterpoise.

Illustr. 148.—Desaguliers' Sponge Hygrometers

Instead of a sponge, Inochodzow employed a highly absorbent schistose rock occurring in Kamchatka. By weighing specimens of this rock (i) after previously heating them to redness, and again (ii) after saturating them with water, he hoped to establish the end-points of a scale on which the humidity of the atmosphere, as measured by the corresponding weight of such a stone, could at any time be specified (*Acta Acad. Imper. Petrop.*, II, 1778). Senebier recommended the weighing of salts of tartar in a sensitive balance for hygrometric purposes (*Journal de Physique*, 1778).

What seems to have been the earliest determination of the dew-point as a means of ascertaining the humidity of the atmosphere was described by C. Le Roy (*Mém. de l'Acad. Roy. des Sciences*, Paris, 1751).

He gradually cooled some water with ice in a vessel until the surface of the latter became fogged with condensed vapour from the surrounding air. The temperature of the water at the instant when this occurred was observed on a thermometer which had been immersed in the water throughout the experiment. The drier the air, the more the water would have to be cooled down before precipitation occurred. Constant sources of error affecting results obtained by this method would be the increase of humidity in the neighbourhood of the water in the vessel, and the necessity of reducing the temperature of the water below the dew-point before precipitation would become noticeable.

Another phenomenon which was utilized in hygrometry at the close of the eighteenth century, and which was destined to find important applications in the nineteenth century, was the cooling produced in a liquid while it is evaporating. This phenomenon seems to have been known to Amontons at the end of the seventeenth century, and it was subsequently rediscovered and described by several other investigators. Thus Richmann observed that when a thermometer was taken out of water into warmer air, its temperature sank below that of either the water or the air, though this effect, he noticed, was less marked in rainy weather (*Novi. Comment. Acad. Imper. Petrop.*, I, 1747). Similar observations were made by Musschenbroek (*Essai de Physique*, §962), and by Mairan (*Dissertation sur la glace*, 1749, pp. 248 f.).

In 1755 William Cullen, Professor of Medicine at Edinburgh and one of Joseph Black's teachers, published *An Essay on the Cold produced by Evaporating Fluids, and of some other means of producing Cold* (*Edinburgh Philosophical and Literary Essays*, II). One of Cullen's students, namely Dobson, had observed that when a thermometer, after having been immersed for some time in spirit of wine of room temperature, was withdrawn and exposed to the air, the mercury always fell two or three degrees (see Black's *Lectures*, ed. Robison, Vol. I, p. 162). Cullen recalled Mairan's account of the phenomenon (he did not then know of Richmann's work), which had already led him to suspect "that water, and perhaps other fluids, in evaporating, produced, or, as the phrase is, generated some degree of cold." He confirmed the student's observation, and made some further trials, in the course of which he worked with an air-thermometer. He found that, by alternately dipping the thermometer into spirit (or moistening it by means of a feather), and allowing it to dry in the air (or, better still, blowing on it with bellows), a remarkable lowering of temperature could be obtained, e.g., from 44° to below 32°. This cooling effect was produced by other liquids besides water, and Cullen arranges these in the order of magnitude of this effect, the list being

headed by "quick-lime spirit of sal. ammoniac." The power of a fluid to produce cold on evaporation seemed to be proportional to its volatility, and to depend on factors hastening evaporation, such as agitation and warmth of the air; Cullen accordingly thought that "we may now conclude, that the cold produced is the effect of evaporation." Upon moistening the bulb of a thermometer with a mineral acid, a considerable *rise* of temperature was observed; but this was obviously attributable to the attraction of these acids for the water in the air, and to the output of heat normally accompanying their dilution. In the course of some further experiments on evapora-tion *in vacuo*, Cullen made the interesting observation that "a thermometer, hung in the receiver of an air-pump, sinks always two or three degrees upon the air's being exhausted," and that it rises when the air is readmitted. He found that spirit of wine, and other liquids, placed in the receiver, showed a fall of temperature when the air was exhausted. He put an open vessel containing some ether in a water-bath, placed the whole in the receiver, and exhausted the air. The water surrounding the ether froze.

A careful study of the same phenomenon was made independently by M. C. Hanov (*Versuche und Abhandlungen der Naturforschenden Gesellschaft in Danzig*, III, 1756, pp. 226–58). In his paper he gives the numerical results of many experiments which he made in this connection. For example, he hung an alcohol thermometer in the air and noted that it stood at 62°; upon being immersed in water it sank to 61°; when withdrawn from the water and suspended nearby, it fell four or five degrees in as many minutes; when replaced in the water it rose once more, but it sank again when taken out; when hung out of the window it fell to 57½°; when fanned, or allowed to swing to and fro in the air, it fell 8° below its initial reading. Hanov confirmed that the cooling was less conspicuous in rainy weather. He varied his experiments by using other liquids besides water. He also placed a thermometer in a glass of water round the outside of which a band of moistened paper was rolled, and he recorded a measurable fall of temperature after three-quarters of an hour. Hanov critically reviews the explanations of this phenomenon suggested by previous writers on the subject. Musschenbroek supposed that an adhering layer of water might attract fire out of the bulb of the thermometer. But if so, Hanov asked, why does the phenomenon not occur when the thermometer is *in* the water? Richmann thought that salts floating in the air might dissolve in the layer of water on the bulb with a corresponding absorption of heat. But is the water on the bulb sufficient in quantity to produce such a fall of temperature by dissolving any known salt? Hanov showed that the cooling phenomenon occurred even in the partial vacuum of an air-pump,

so that the air was probably not responsible; rather he was convinced that it was entirely an effect of evaporation. His experiments showed that the cooling was most marked when the air was dry, when the thermometer was in a current of air, when the bulb and the water were appreciably warmer than the air in which the evaporation took place, and when this evaporation was allowed to continue for an appreciable time. He regarded the cooling which must accompany the evaporation of moisture from leaves as a providential means of protecting plants from excessive heat in summer.

Upon this property of evaporating liquids are based those hygrometers in which the humidity of the atmosphere is inferred from the difference between the readings of two adjacent thermometers, of which one has its bulb kept always moist. An anticipation of the wet and dry bulb hygrometer was described by Leslie in 1799 (*Nicholson's Journal*, Vol. III). His instrument consisted essentially of a U-tube, each limb of which ended in a closed bulb. The tube contained coloured sulphuric acid; this was brought to the same level in the two limbs, and then one of the bulbs was covered with wet muslin. The cooling due to evaporation from the muslin caused a contraction of the air, and a consequent rise in the level of the liquid in the corresponding limb of the tube. The drier the air, the greater the rate of evaporation, and the more appreciable this displacement of equilibrium would be.

Proposals to ascertain the humidity of the atmosphere by the application of electrical criteria were occasionally made towards the close of the eighteenth century. The electrical conductivity of the atmosphere increases with its moisture. Accordingly, Volta, in 1790, proposed to test the humidity of the air by charging up an electrometer to a given degree, and noting the time required for the whole charge to be conducted away by the air (*Mem. di Mathem. e Fisica della Soc. Ital.*, V). Another proposal was to find the average number of revolutions of a uniformly working electrical machine, between the passage of two successive sparks when the conductors were set a given distance apart (*Hist. Acad. Theodor. Palat.*, VI).

CHEMISTRY (I)

MANY years elapsed after the foundation of modern physics before chemistry dispensed with its medieval outfit and, partly under the guidance of Robert Boyle but more especially through Lavoisier, whom he had influenced, entered on the scientific path of investigating the composition of bodies. Boyle had finally fixed the conception of a chemical element, and provided a secure foundation for analytical chemistry, though it needed a Lavoisier to secure the general acceptance of Boyle's ideas. He had also made an experimental study of the phenomena of combustion, and an attempt to explain them. The first part of this enterprise was greatly advanced by Boyle and his contemporaries, inasmuch as they provided a large accumulation of experimental data relating to the process of combustion. But the second part of the enterprise, namely the explanation of the phenomena of combustion, was not so successful. The phlogiston theory of Stahl, consequently, captured the chemical world and held it until the time of Lavoisier. Even after Lavoisier had seriously challenged the phlogiston theory chemists like Priestley, on whose support he relied, would not abandon it. But with Berthollet, Proust, Dalton, Berzelius, and Gay-Lussac a new generation of chemists made its appearance. Continuing the work of Black, they introduced the era of quantitative research in chemistry.

A. THE PHLOGISTON THEORY

While Boyle and some of his British contemporaries were endeavouring to solve the problems of combustion, calcination, and respiration on the lines that these phenomena were conditioned by something contained in the air and taken from it during these processes, a different line of thought was pursued on the Continent, and dominated chemical theory during a considerable part of the eighteenth century. This rival mode of explanation is known as the phlogiston theory. According to this theory all combustible substances contain an inflammable element which is given off during combustion, calcination, and respiration, and is absorbed by the surrounding air. This inflammable element, or principle of fire, was called phlogiston. The idea of a caloric stuff was an old and familiar working concept. Fire was one of the traditional elements, and even modern thinkers like Descartes and Boyle believed in the existence of a special kind of fire-particles. Possibly the newly

Georg Ernestus Stahl, onoldo Francus,
Med. Doct. h. t. Prof. Publ. Ord. Hall.

Scheele

Cavendish
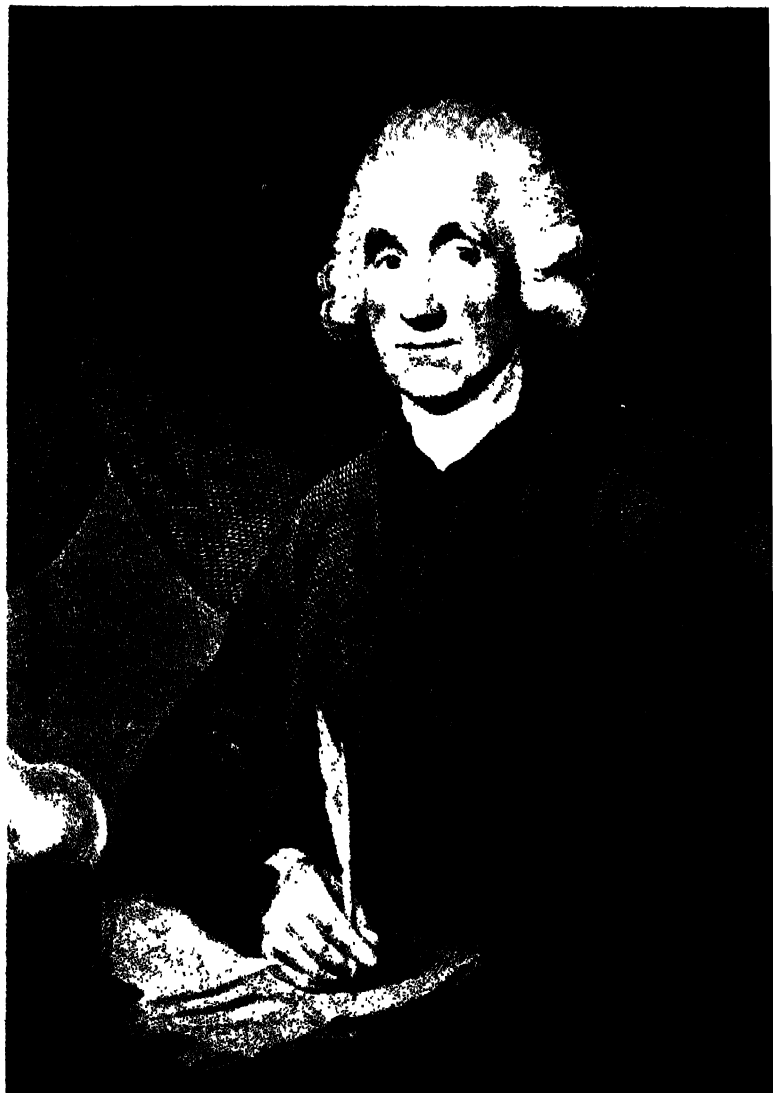
Joseph Priestley
(By courtesy of the Science Museum, London)

discovered phosphorus also tended to encourage the idea. The main cleavage of opinion was that some held that some such caloric element was contained in the air and taken from it to support combustion, calcination, and respiration, whereas others held that this caloric stuff was contained in the burning, calcined or breathing bodies and was given off by them during these processes to be absorbed by the air. Appearances could be cited in support of either of these rival views. The fact that the former theory eventually triumphed must not blind one to the usefulness of the phlogiston theory in rendering intelligible numerous chemical phenomena. It seemed simple and reasonable to suppose, for instance, that the vitiation of an enclosed volume of air, in which combustion or respiration had taken place, was due to its absorption of something given off by the breathing or burning bodies; or again, that the restoration of such vitiated air, when a plant was growing in it, was due to the plant's reabsorption of the phlogiston from the air.

The founders of the phlogiston theory were the German chemists Becher and Stahl; and among those who adopted the theory were such famous chemists as Scheele, Priestley, Macquer, Cavendish, Meyer and, for a time at least, Black and Berthollet.

BECHER AND STAHL

J. J. Becher (1635–82) was an iatro-chemist, who enjoyed the patronage of various German princes. In his *Subterranean Physics* (1669) he rejected all the traditional elements and principles except water and earth. But he distinguished three kinds of earth. One of these he named "oily earth" (*terra pinguis*), and he argued that it must be contained in all combustible bodies. Fire had long been regarded as a universal resolvent separating compound bodies into their constituents. A combustible body, it was argued, must consequently be composite. During combustion or calcination "oily earth" is expelled, and that is why only "stony" or "vitreous earth" is left behind. Moreover, it was urged, the smaller the amount of such residue a substance left behind after combustion or calcination, the more "oily earth" must there have been in its original constitution. Hence charcoal, which leaves but a very slight amount of ash, was regarded as almost pure "oily earth." Becher's ideas were taken over and extended by G. E. Stahl (1660–1734), Professor of Medicine and Chemistry at the University of Halle, and subsequently in Berlin. It was Stahl who gave vogue to the term "phlogiston" (which had already been used by Boyle in another connection), in the place of Becher's "oily earth," as the principle of inflammability. According to Stahl, when metals are calcined they give off the

contained phlogiston, which is absorbed by the surrounding air. When ores are converted into metal by heating with charcoal, they absorb some of the phlogiston given off by the charcoal, which, as just explained, was regarded as almost pure phlogiston. Free air was admitted to be necessary for combustion, etc., but only in order to absorb the phlogiston given off during combustion, etc. For there could be no combustion, etc., without the expulsion of phlogiston, and the phlogiston could not leave the combustible body if there were no free air to absorb the phlogiston.

(See L. J. M. Coleby, *Studies in the Chemical Work of Stahl,* 1938, Ph.D. Thesis, Library of the University of London.)

POTT, MACQUER, ETC.

One of the earliest problems confronting the phlogiston theory was how to account for the fact that calces were heavier than the metals from which they had been produced, in spite of the alleged loss of the phlogiston during calcination. Stahl gave no decisive solution of this problem. Some chemists held that the gain in weight may be due either to an increase in the density of the calcined substance or to its absorption of particles of air. There were, however, others who attributed the lighter weight of the metal to the levity (or negative weight) of phlogiston. According to this view phlogiston gives a certain amount of buoyancy to a substance containing it; so when the phlogiston has been expelled by heating the substance becomes heavier. This ingenious idea was not widely accepted. Even Scheele and Priestley could not believe that any material substance had levity instead of gravity. They preferred to plead ignorance of the real nature of phlogiston while believing in its existence. This left it open to them to use it when they found it helpful to do so without feeling unduly embarrassed about things they could not explain. However, attempts continued to be made to account for the alleged diminution of gravity under the influence of phlogiston. Thus, in 1780, J. Elliot suggested that this action of phlogiston may be due to its "weakening the repulsion between the particles and aether," and thereby diminishing their mutual gravitation (*Philosophical Observations on the Senses . . . and an Essay on Combustion . . .*, 1780, p. 122). Another and simpler explanation was put forward by P. J. Macquer (1718-84). According to Macquer, calces are metals deprived of their phlogiston but charged with gas, and it is this gas that accounts for the increased weight of metals on calcination (*Dictionnaire de Chimie*, Paris, 1778, articles on "Chaux

metalliques" and "Combustion"). He thus appears to have believed that, even if phlogiston has weight, the weight of the gas taken up by a metal on calcination exceeds the weight of the phlogiston lost in the process

(See J. R. Partington and D. McKie, "The Levity of Phlogiston," *Annals of Science*, 1937, Vol. 2, pp. 361–404, and "The Negative Weight of Phlogiston," *Annals of Science*, 1938, Vol. 3, pp. 1–58; L. J. M. Coleby, *The Chemical Studies of P. J. Macquer*, London, 1938.)

LAVOISIER

The story of the various chemical discoveries which eventually led to the displacement of the phlogiston theory by the new chemistry is told in the rest of this chapter. Here it need only be added that, already in 1774–75, Pierre Bayen (1725–98), in his investigations into mercuric oxide, found that this calx could be converted into metal by heating, even without the addition of carbon which was alleged to provide the necessary phlogiston, and that air was evolved in the process of changing the calx into metal. He concluded that the calx was composed of metal and air, and that the combination of the air with the metal was the cause of its increased weight on calcination. Moreover, he realized that his observations were inconsistent with the phlogiston theory; and he appears to have rejected it even before Lavoisier did so (*Observations sur la Physique*, Vol. III, 1774, pp. 127, 278; Vol. V, 1775, p. 147; Vol. VI, p. 487). It was Lavoisier, however, who initiated the most effective opposition to phlogiston a few years later. In 1783 Lavoisier attacked the theory in his *Reflexions on Phlogiston*, from which the following remarks may be quoted. "Chemists have made of phlogiston a vague principle which is not rigorously defined, and which consequently adapts itself to all explanations into which it may be introduced. Sometimes this principle is heavy, and sometimes it is not; sometimes it is free fire, sometimes it is fire combined with the earthy element; sometimes it passes through the pores of vessels, and sometimes they are impenetrable for it. It explains at once causticity and non-causticity, transparency and opacity, colours and the absence of colours. It is a veritable Proteus which changes its form at every instant" (*Mém. de l'Acad. Roy. des Sciences*, 1783, p. 523).

(See J. H. White, *A History of the Phlogiston Theory*, 1932; D. McKie, *Antoine Lavoisier*, 1935.)

B. THE STUDY OF GASES BEFORE LAVOISIER

A real insight into the nature of combustion was first made possible by Priestley's study of gases and Scheele's discovery of the two constituents of atmospheric air. Until the time of Van Helmont the gases especially known were hydrogen and carbon dioxide. But even these were not always clearly distinguished from one another

or from atmospheric air. In fact, there was a tendency to identify all kinds of gases with air and to attribute their differences from each other to different admixtures in the air. The successful study of gases only began after the invention of suitable means of collecting and storing them. Stephen Hales (*Vegetable Staticks*, 1727) discovered a method of collecting the "air" over water and conducting it into a separate "receiver" also inverted over water. This pneumatic trough is shown in Illustr. 153. Hales' method of collecting and storing gases
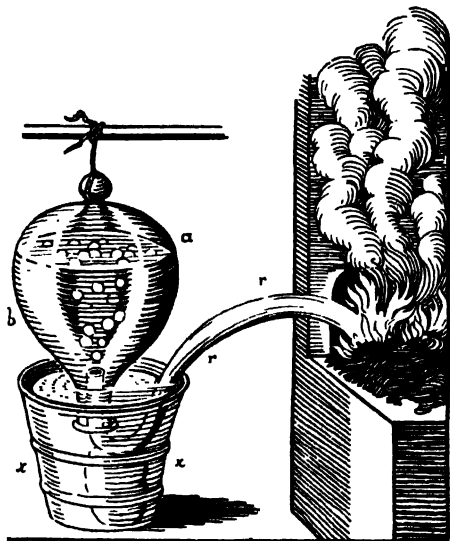
Illustr. 153.—Hales' Improved Pneumatic Trough

over water had one serious disadvantage: it could not be used for the study of such gases as are soluble in water—for instance, ammonia and hydrogen chloride. It was only after Cavendish and Priestley had shown, respectively, how gas could be stored and even collected over mercury (instead of water) that such gases could be discovered and studied. These improvements in technique, and the discoveries which the aforementioned investigators made by means of them, came to full fruition in the work of Lavoisier, who had the clearest ideas of the nature of gases and was the first to describe oxygen and hydrogen as elements.

BLACK

Among the pioneers who used quantitative methods in chemistry, and thereby showed, at least implicitly, their faith in the principle

of conservation of matter, Black holds an honourable place. Joseph Black (1728–99) was born, of Scottish parents, in Bordeaux, went to school at Belfast, studied medicine and chemistry at the Universities of Glasgow and Edinburgh, and in due course also taught these subjects at these universities in succession. In 1754 he was awarded the degree of M.D. for a Latin dissertation which contained a very important section on chemistry. This section was expanded and published in 1756, in English dress, in the *Edinburgh Physical and Literary Essays*, under the title of "Experiments upon Magnesia Alba, Quicklime, and Some Other Alcaline Substances" (No. 1 of the *Alembic Club Reprints* gives this paper in convenient form).

Medical interest first prompted Black to study "magnesia alba," which seemed to him to be a mild alkali. But when he let lime-water act on it, no caustic solution was left, such as mild alkalis usually leave. So he tried to reduce it by heating, and found that the ounce of magnesia had lost seven-twelfths of its weight through heating. The residue dissolved in the common acids, and gave the same salts as ordinary magnesia alba; but, unlike this, it dissolved without the usual effervescence, and did not precipitate lime-water. The first thing he did was to try to trace the lost weight of the heated magnesia to the volatile parts. Heating a weighed amount of magnesia in a retort, he condensed the issuing vapours and weighed the water so obtained. This, however, accounted for but a small part of the weight lost by the magnesia. The rest of the lost weight, Black concluded, must be due to the uncondensable air in the vapours which had issued from the heated magnesia. As the calcined magnesia did not effervesce with acids, it was clear that the air had escaped from it. Again he calcined a weighed quantity of magnesia, noted the precise loss of weight, dissolved the calx in a sufficient quantity of spirit of vitriol, and re-precipitated it by adding alkali. The magnesia thus reproduced had practically its original weight, effervesced with acids, and precipitated lime-water. The recovered weight and other properties must have been due to its absorption of air obtained from the alkali. This, he thought, was quite in accordance with the observation made by Hales, namely, that alkaline salts when acted on by acids yield up the air that was fixed in them. This was confirmed by Black's experiment of saturating a weighed quantity of pure fixed alkaline salt (sodium carbonate) with a weighed amount of diluted oil of vitriol, and showing that the mixture had lost in weight. Taking next a weighed amount of magnesia, and dissolving it in the same acid, he found that this mixture, too, had lost in weight. He then calcined an equal quantity of magnesia, weighed it, and dissolved it in the same acid as before. He found that there was no loss of weight in this case, and that the amount of acid required to

dissolve the calcined magnesia was practically the same as in the preceding experiment. The difference between uncalcined and calcined magnesia, Black concluded, is simply that the former contained "a considerable quantity of air." Extending his experiments to chalk and quicklime, Black satisfied himself that they are related in the same way as magnesia alba to calcined magnesia, and that the process of causticization of mild alkalis by lime consisted in a transfer of "air" from the alkali to the lime.

Black considered the nature of this "air," and was of the opinion that it was different from atmospheric air. Quicklime, for instance, does not attract ordinary air, but does attract this special "air" (or "fixed air," as he called it, adopting a term used by Hales). He also maintained, soon afterwards, that it was this "fixed air" that caused suffocation in mines and grottos, and was evolved in vegetable fermentations; and that it differed from the "air" produced by the solution of metals in acids, and resembled air which had been vitiated by combustion or respiration.

The next advance in the study of gases was made by Priestley, a younger contemporary of Black.

PRIESTLEY

Joseph Priestley (1733–1804) was born near Leeds. He studied theology, and worked sometimes as minister of religion, sometimes as schoolmaster, and sometimes as private tutor. Owing to his critical attitude towards the Church of England, and the liberality of his religious views, he lived a troubled life, and eventually migrated to North America, where he died. Notwithstanding his lack of scientific training in his early years, he succeeded in laying the foundations of the chemistry of the gases, and thereby prepared the ground for the work of Lavoisier. His exceptional skill in experimentation made amends for his lack of a thorough training in science during his youth. His chemical experiments and results are described in his *Experiments and Observations* (6 vols., 1774–86).

Priestley's first chemical researches dealt with what he called "fixed air" (carbon dioxide). He obtained this gas either from breweries, since it can be produced by fermentation, or by pouring acid on chalk, or by the action of "oil of vitriol" on common salt. He investigated at the same time the solubility of "fixed air" in water; and showed how artificial mineral water may be produced by impregnating ordinary water with "fixed air." This invention was a characteristic expression of Priestley's faith in the practical utility of scientific knowledge. He was led to this application of science to daily needs by his experiments on "fixed air," and his invention was esteemed so highly that the naval authorities introduced it on some

men-of-war in the hope of resisting the ravages of scurvy. According to some writers, it was in recognition of his invention of soda water that the Royal Society, in 1773, awarded the Copley Medal to Priestley. This view, however, is erroneous. The Copley Medal was given to him for his work on various "airs," of which he gave an account in his paper published in the *Philosophical Transactions* for 1772 (pp. 147–264).

In 1771, Priestley observed that sprigs of mint, placed in air that had been tainted by animal respiration, grew in a surprisingly vigorous manner. He had for some time been considering what provision nature had made for restoring the freshness of the air that was being continually vitiated by combustion and respiration. Evidently, he argued, there must be some means, else the whole atmosphere would in course of time become unfit to sustain life. The observation about the remarkable growth of the sprigs of mint suggested to him that plants, instead of polluting the air as breathing animals did, had the reverse effect of tending to keep the air wholesome. "In order to ascertain this," he wrote, "I took a quantity of air, made thoroughly noxious, by mice breathing and dying in it, and divided it into two parts; one of which I put into a phial immersed in water; and to the other (which was contained in a glass jar, standing in water) I put a sprig of mint. This was about the beginning of August 1771, and after eight or nine days, I found that a mouse lived perfectly well in that part of the air, in which the sprig of mint had grown, but died the moment it was put into the other part of the same original quantity of air, and which I had kept in the very same exposure, but without any plant growing in it" (*Experiments and Observations*, Vol. I, 1774, p. 86). Priestley repeated and confirmed these observations in various ways, and concluded that it was highly probable "that the injury which is continually done to the atmosphere by the respiration of such a number of animals, and the putrefaction of such masses of both vegetable and animal matter, is, in part at least, repaired by the vegetable creation. And, notwithstanding the prodigious mass of air that is corrupted daily by the above-mentioned causes; yet, if we consider the immense profusion of vegetables upon the face of the Earth, growing in places suited to their nature, and consequently at full liberty to exert all their powers, both inhaling and exhaling, it can hardly be thought, but that it may be a sufficient counterbalance to it, and that the remedy is adequate to evil" (*idem*, pp. 93 f).

Priestley's endeavours were especially directed to the discovery and study of new "airs" (or gases), many of which he obtained from acids. In 1772 he isolated "nitrous air" (nitric oxide) by the action of nitric acid on such metals as iron, copper, silver, etc., collected it

over water, and examined its properties. In the same year he collected "marine acid air" (hydrogen chloride) over *mercury* (instead of over water). Already in 1766 Cavendish had *stored* gases over
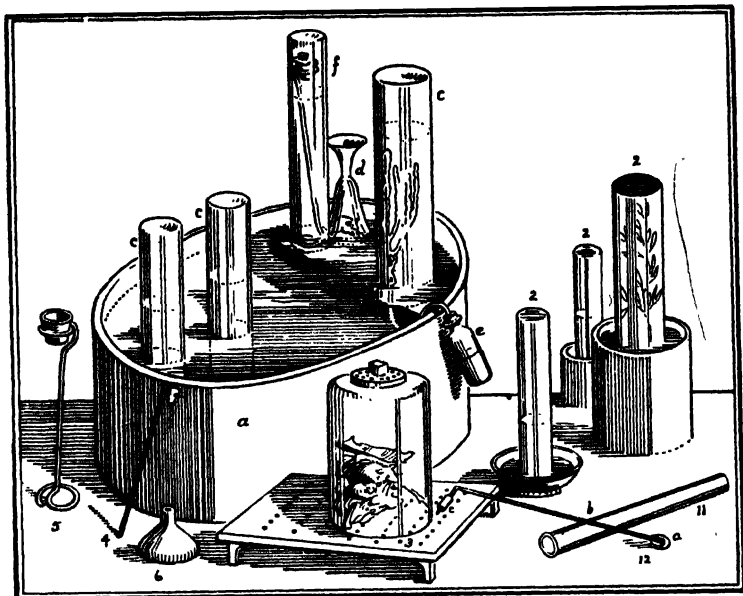
Illustr. 154.—Priestley's Apparatus (I)

Pneumatic trough (*a*), at the right end of which there is a shelf of flat stones, just below the surface of the water, and supporting various vessels (*c, d, f*); *c, c* are jars for collecting air; 2, 2 are jars containing air over water, the jar on the right contains a plant; *d* is a beer-glass holding an air over water, and a mouse to test the respirability of the air; 3 is a receiver for housing mice, it is open above and below, stands on a perforated tin-plate, and is kept in position by a weight on the top; 4 is a wire for extracting corks from phials in airs collected in jars; 5 is a stand for supporting substances inside jars of airs, as in *f*; 6 is a glass funnel used for passing of an air from one vessel to another; *e* is a phial for producing airs (by the solution of metals in acids, or in some other way), and connected by a glass tube to the jar in which the air is collected; 11 is a cylindrical glass vessel; 12 is a wire (*b*) holding a wax candle (*a*) at the right end, "turned up in such a manner as to be let down into the vessel with the flame upwards" (and used to ascertain whether the air contained in the vessel would support the flame), and, at the left end, another candle (*c*), to be used in the case of jars standing over water—this candle could be withdrawn as soon as the flame went out, and before any smoke mixed with the air in the jar.

mercury, but Priestley *collected* them in this way; and to this new method he owed many of his discoveries in pneumatic chemistry. Thus, in 1773, he discovered by its means "alkaline air" (ammonia), and, in 1774, "vitriolic acid air" (sulphur dioxide). "Marine acid

air" he obtained at first by heating copper with spirits of salt. Afterwards he prepared this gas by heating "spirits of salt" alone,
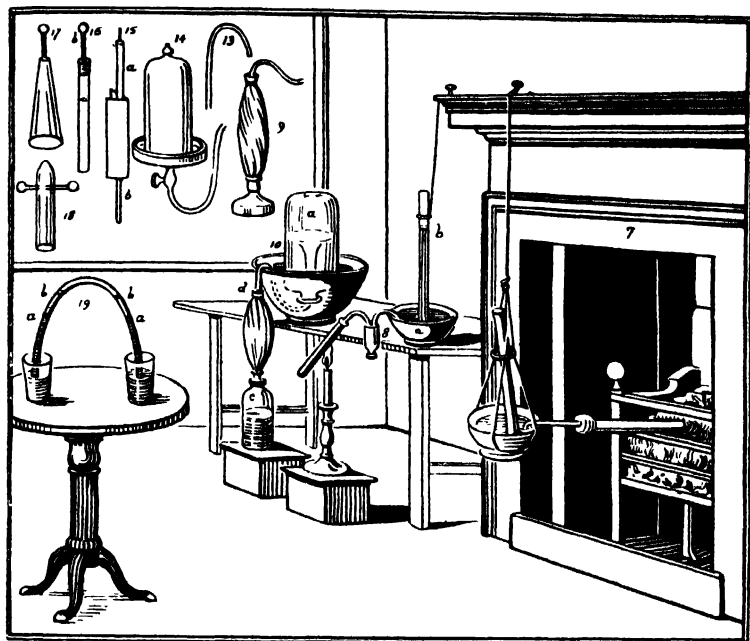
Illustr. 155.—Priestley's Apparatus (II)

7. Apparatus for collecting airs, produced in heated gun-barrel, over mercury. A tobacco-pipe stem or a glass tube was luted to the open end of the gun-barrel to convey the products into the receiver.

8. Another apparatus for collecting airs over mercury; in the middle there is a trap to collect any moisture driven over by the heat applied.

9. A bladder, fitted with delivery tube and funnel, for transfer of an air standing over water to a vessel standing over mercury, or to "any other situation."

10. Apparatus for impregnating liquids with an air, produced in c and compressed in a bladder via leather tube d into the liquid in vessel a (inverted over a bowl of the same liquid).

13. A syphon for withdrawing air from vessels and adjusting the level of the water in the vessels.

14. An evacuated receiver to contain substances that were to be exposed dry to airs conveyed into the receiver from vessels standing over water.

15. An eudiometer for testing the "goodness" of a small amount of air.

16, 17, 18. Various forms of apparatus for passing electric sparks through airs or liquids.

19. An apparatus for passing electric sparks through air confined over mercury in a tube, each end of which stood in a vessel containing mercury.

or by letting "oil of vitriol" act on common salt. "Alkaline air" (ammonia) was obtained at first by heating "volatile spirit of sal

ammoniac" (aqueous solution of ammonia), and later by heating slaked lime with sal ammoniac. In 1772 Priestley also prepared nitrous oxide (or laughing gas, N_2O). In 1776 he prepared "nitrous [nitric] acid vapour" (nitrogen peroxide, NO_2), in an impure state, by the action of nitric acid on bismuth, and, since it dissolved in water and corroded mercury, collected it by displacement of air. He observed, among other things, that its brown colour was intensified by heating. In 1785 he prepared carbon monoxide (CO) by heating scales of iron (iron oxide) with charcoal; but he mistook it for "inflammable air" (H_2). (In 1776 Lassone had obtained carbon monoxide by heating zinc oxide with charcoal, and also by heating Prussian blue in a pistol barrel. He described it, in *Mém. de l'Acad. Roy des Sciences*, Vol. XC, as "an inflammable air of quite a peculiar character."[1] Most important of all was Priestley's discovery of "dephlogisticated air" (oxygen), in 1774. He obtained it by heating red oxide of mercury. (The same discovery was also made independently by Scheele about the same time.) Priestley's discovery of oxygen was first announced by him on March 15, 1775, in a letter to Sir John Pringle, President of the Royal Society, and the letter was read to the Society on May 25, 1775 (*Phil. Trans.*, 1775, p. 387). In this letter Priestley gives an account of his experiments on heating various substances by means of a burning-lens, and collecting the "airs" evolved. He says that he observed that different substances yielded different kinds of air by this method, and "the most remarkable of all the kinds of air that I have produced by this process is one that is five or six times better than common air for the purpose of respiration, inflammation, and, I believe, every other use of common atmospherical air. As I think I have sufficiently proved that the fitness of air for respiration depends upon its capacity to receive the phlogiston exhaled from the lungs, this species may not improperly be called *dephlogisticated air*. This species of air I first produced from *mercurius calcinatus per se*, then from the red precipitate of mercury, and now from red lead." He found that "a quantity of this air required about five times as much nitrous air to saturate it as common air requires." Further, "a candle burned in this air with an amazing strength of flame; and a bit of red-hot wood crackled and burned with a prodigious-rapidity, exhibiting an appearance something like that of iron glowing with a white heat and throwing out sparks in all directions." A mouse lived much longer in this air than in common air. And when Priestley himself inhaled some of the "dephlogisticated air" his "breast felt peculiarly light

[1] It was not until 1801 that Cruickshank showed that this "peculiar kind" of inflammable air was an oxide of carbon, not hydrogen. See *Nicholson's Journal*, Vol. V.

and easy for some time afterwards." Hence his subsequent suggestion that "dephlogisticated air" might be of use in the treatment of diseases of the lungs. This suggestion is contained in his *Experiments and Observations* (Vol. II, pp. 101 f.).

Priestley published his *Experiments and Observations*, Vol. II, late in 1775, and the work includes another account of his discovery of oxygen. A few sentences may be quoted from it here.

"At the time of my former publication, I was not possessed of a burning lens of any considerable force. . . . But having afterwards procured a lens of twelve inches diameter, and twenty inches focal distance, I proceeded with great alacrity to examine, by the help of it, what kind of air a great variety of substances, natural and factitious, would yield, putting them into . . . vessels . . . which I filled with quicksilver, and kept inverted in a bason of the same. With this apparatus . . . on the 1st of August, 1774, I endeavoured to extract air from *mercurius calcinatus per se*; and I presently found that, by means of this lens, air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it. But what surprised me more than I can well express, was, that a candle burned in this air with a remarkably vigorous flame" (*Experiments and Observations*, Vol. II, 1775, pp. 33 f.).

Priestley had occupied himself with electricity long before he turned to the study of the chemistry of gases (see Chapter X). In fact it was his interest in electricity that had led to his election as a Fellow of the Royal Society (see W. C. Walker in *Isis*, 1933). His *History and Present State of Electricity with Original Experiments*, published in 1767, had found great favour. It is noteworthy how Priestley applied his knowledge of electricity in his experimental researches on gases. In 1773–74 he confined some atmospheric air in a glass tube over water coloured blue with litmus, and passed an electric spark repeatedly through the air. The result was a diminution in the volume of the air, and a change in the colour of the water from blue to red. In the case of ammonia gas (or "alkaline air," NH_3) something peculiar happened: after repeated sparking it *increased* in volume, instead of diminishing, as had happened in the case of the atmospheric air. The "alkaline air," or ammonia gas, Priestley realized, must have undergone some far-reaching chemical change. He wrote: "I took the electric explosion in a small quantity of alkaline air . . . and observed that every stroke added considerably to the quantity of air; and when water was admitted to it, just so much remained unabsorbed as had been added by the explosions. I then took about an hundred explosions of the same jar, in a larger quantity of alkaline air; after which so much of it remained un-

absorbed by water that I could examine it with the greatest certainty. It neither affected common air, nor was affected by nitrous air, and was as strongly inflammable as any air that I had ever procured" (*Experiments on Air*, Vol. II, 1775, pp. 239 f.).

Priestley also introduced the method of analysing gases by explosion with oxygen. He mixed inflammable gases with oxygen over mercury. An explosion was then produced by means of an electric spark, and the residue was examined. In this way Priestley discovered that the "inflammable air," which is produced when alcohol vapour is passed through a red-hot tube, or when finery cinder (iron oxide) is heated with charcoal, leaves, when mixed with oxygen and exploded, a residue of "fixed air" (CO_2), whereas the inflammable gas (H_2) produced from iron and sulphuric acid leaves no such residue when exploded in the same way (*op. cit.*, Vol. I, ed. 1790, pp. 309 f.).

All these discoveries of Priestley's were of the utmost importance for the progress of chemistry. But he expressed his results in terms of the phlogiston theory. Combustion was described by him as consisting in a loss of phlogiston, which is absorbed by those airs which support combustion, and is absorbed by them all the more, the less phlogiston they themselves contain. The gas now called oxygen supports combustion best of all, because, according to Priestley, it contains no phlogiston whatever. So he called it "dephlogisticated air." "Inflammable air" (hydrogen), on the other hand, was for a time identified by Priestley with pure phlogiston. This view was first suggested by Richard Kirwan (1733–1812), in *Phil. Trans.* for 1782 (Vol. LXXII, pp. 195 f.), mainly in consequence of the information which Priestley had given him privately about his experiments in converting calces into metals by heating them in "inflammable air"—experiments subsequently described by Priestley in his *Experiments and Observations* (Vol. VI, 1786, p. 14). Priestley, however, soon abandoned the view that "inflammable air" was pure phlogiston. After observing (with Warltire) that on the explosion of "inflammable air" with common air a dew was deposited, which Cavendish had shown to be water, Priestley formed the opinion that "inflammable air" was phlogiston united with water.

Atmospheric air, according to the phlogiston theory, is a mixture of "dephlogisticated air" (oxygen) and "phlogisticated air" (nitrogen). When combustion takes place in it the atmospheric air is impregnated with additional phlogiston, and is thereby changed into "phlogisticated air." Like his contemporaries, Priestley did not sufficiently consider the difficulty presented to the phlogiston theory by the fact that in certain processes of combustion the "dephlogisticated air" was entirely consumed without giving off any "phlogisticated air."

In the course of his researches into inflammable air, Priestley

sought to ascertain "the quantity of phlogiston that enters into the composition of the several metals" when these are obtained from their calces by heating in "inflammable air." He fully expected such metals to be *heavier* than their calces by the quantity of inflammable air absorbed in the process. Had he succeeded in carrying out these experiments to his satisfaction, his faith in the phlogiston theory would have been shaken by the discovery that the revived (or pure) metals were *lighter* than their calces. But his experiments appeared to him to be indecisive, because the calces appeared to have partially sublimed in the vessels in which they were heated. He was never certain whether he had a pure calx to begin with, or a pure metal at the end of the experiment. He did not know "the allowance that was to be made for the inflammable air which entered into that part of the calx which was only partially revived; and it was not easy to revive the whole of any quantity of calx completely" (*op. cit.*, Vol. VI, 1786, p. 14). The experiments, then, did not definitely show whether a metal is or is not heavier than its calx.

(See D. McKie, "Joseph Priestley," *Science Progress*, 1933, Vol. 28, pp. 17–35.)

VOLTA

The method of sparking gases in a closed vessel appears to have been originated by Priestley, who described it, and the apparatus used for it, in his *Experiments and Observations*, Vol. I, 1774. But the application of this method to *mixed* gases is due apparently to Alessandro Volta (1745–1827), whose first experiments on the explosion of mixed gases in closed vessels by means of an electric spark were described by him in *Scelta d'Opuscoli Interressanti*, Vol. XXXI, p. 3 (Milan, 1777), and again in a letter to Priestley, written on September 2, 1777, and published in the same year in *Scelta d'Opuscoli . . .*, Vol. XXXIV, p. 65. Volta had been interested for some time in the explosion of "inflammable air" (hydrogen) and "dephlogisticated air" (oxygen) in open vessels by

Illustr. 156.—Volta's Apparatus for Exploding Gas

means of a candle flame. He had also attempted to construct a *pistolet* or *petit fusil* to be worked by the force developed in such explosions. He
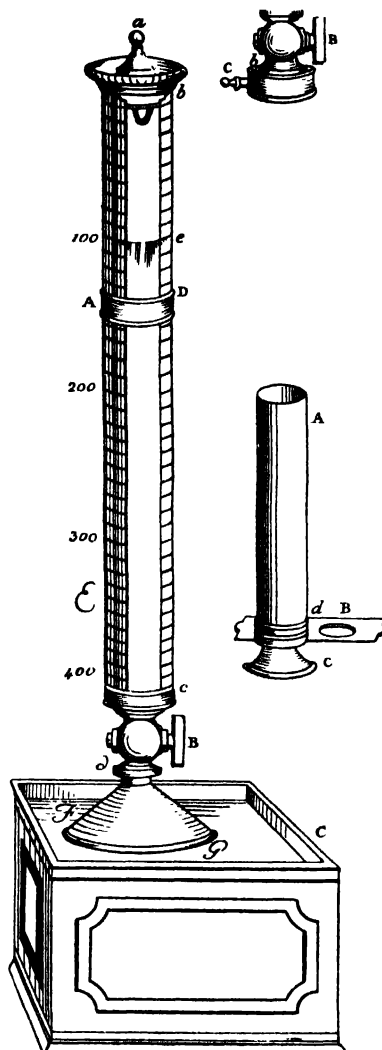
Illustr. 157.—Volta's Eudiometer

Gas was collected over water in trough C, via a funnel, and exploded with oxygen in the graduated vessel E. An insulated wire, *a*, passed through the metal cap *b*; and the spark gap was formed between the end of the wire and the cap. The height of the column of mercury was read on the scale with the aid of the ring AD. The "goodness" of the air was estimated by the proportion of oxygen it contained.

then exploded, by means of an electric spark, mixtures of "inflammable air" with common air in glass vessels closed with a cork, which was expelled by the force of the explosion. And he used this method in order to study the changes in volume consequent on such explosions. His apparatus consisted of a graduated glass tube, one end of which opened out like a funnel, the other end being closed with a gummed cork through which two metal wires passed to form the points of the spark-gap (see Illustr. 156). The tube was filled with water and inverted over water. Eight measures of common air were conveyed into the tube, and then one measure of "inflammable air." When the spark was passed no air escaped, but the water rose. More "inflammable air" was added, and the mixture sparked until the total volume diminished by nearly $\frac{1}{3}$ part. Volta also found that the strongest explosion was produced in such a mixture when there were four parts of "inflammable air" to eleven parts of common air. In this case the residue was as "phlogisticated" as it could be. Volta also appears to have made some experiments with "dephlogisticated air" instead of common air. And he suggested the use of this method for constructing a eudiometer, for measuring the goodness of air (Illustr. 157).

RUTHERFORD

Daniel Rutherford (1749–1819), a pupil of Black's and President of the Royal College of Physicians of Edinburgh, 1796-98, is credited with the discovery of nitrogen. He carried out this work at the suggestion of Black, and presented it as a thesis for his M.D. degree. This was his only work in chemistry. It bore the title *Dissertatio Inauguralis de acre fixo dicto, aut mephitico* (Edin., 1772). His work is described in terms of the phlogiston theory; and his experiments do not appear to have been very extensive. He showed that a mouse, breathing in a limited quantity of air until it died, diminished the air by one-sixteenth; that one-eleventh of the residual air was absorbed by alkali; and that the final residue extinguished a candle. In similar experiments, carried out with a burning candle and kindled charcoal, the residue, after "fixed air" had been removed by alkali, showed similar properties.

This "noxious air" was described by Rutherford as a compound of pure phlogiston and air. The same "noxious air" was obtained, he said, when metals were calcined in common air. This he regarded as a further proof of its nature, since it was produced only from bodies containing phlogiston.

Thus Rutherford's claim to the discovery of nitrogen is that he isolated it from air by removing the oxygen by combustion, and

then by means of alkali removing any fixed air produced in the combustion. On the other hand, he did not recognize that his "noxious air" was a distinct kind of elementary gas, but regarded it merely as common air combined with phlogiston.

(See D. McKie, "Daniel Rutherford and the Discovery of Nitrogen," in *Science Progress*, 1935, Vol. 29, pp. 650–60.)

SCHEELE

Carl Wilhelm Scheele (1742–86) was born at Stralsund, in Pomerania, which at that time belonged to Sweden. From the age of fourteen he worked in pharmacies. In 1770 he settled at Upsala, where he was befriended by Bergman. In 1775 he was elected a member of the Academy of Sciences at Stockholm, and in the same year he opened a pharmacy of his own at Köping on Lake Malar. He managed somehow to do an extraordinary amount of research work. Whereas Priestley confined himself mainly to the chemistry of the gases, Scheele spread himself over practically the whole field of chemistry. Overwork ended in his early death.

"It is the object and chief business of chemistry," according to Scheele, "skilfully to separate substances into their constituents, to discover their properties, and to compound them in different ways" (*Chemical Treatise on Air and Fire*, 1777; English version in Dobbin's edition of *The Collected Papers of C. W. Scheele*, 1931, p. 89). As the study of combustion had led to so many difficulties and contradictions, he determined to carry out many experiments independently in order to fathom the mysteries of the phenomena of combustion. He soon realized the impossibility of solving the problems of combustion without a close study of the air. These problems, accordingly, occupied his attention during the years 1768–73; and he gave an account of his experiments and results in his *Chemical Treatise on Air and Fire* (1777). Scheele first determined the properties which distinguish air from other gases, and then carried out a series of experiments to show that air is composed of two different gases. His method consisted in taking a definite quantity of air and treating it with some substance which absorbed one part of the air and left the other part, which in various experiments had the same properties and nearly the same volume. Thus, for instance, he put a solution of liver of sulphur into an air-filled flask, which he closed, inverted, and placed over water. He left the flask in this position for fourteen days, and then removed the cork while still under water. The water immediately entered the flask, and it appeared that a fourth to a third part of the air had been absorbed. Approximately the same diminution in the volume of the enclosed air took place when Scheele repeated the experiment with phosphorus, iron filings,

or a suitable compound of iron, instead of liver of sulphur. But when he burnt some hydrogen in a quantity of air similarly enclosed in a flask (Illustr. 158) the volume of air was diminished by a fifth only.

Scheele prepared oxygen in several ways. One way was to mix concentrated sulphuric acid with some finely ground "manganese" (i.e., pyrolusite, native manganese dioxide), and heat the mixture in a small retort. An empty bladder served as a receptacle for the gas (Illustr. 159).

As soon as the bottom of the retort was red-hot a gas passed into the bladder and gradually distended it. Scheele had once taken a sample of such gas, which he had prepared by heating acid of nitre, and plunged a small lighted candle into it. "No sooner was this done than the candle began to burn with a big flame and emitted such a bright light that it dazed the eyes." When he mixed the same gas with the residue of the air in which, in the above experiments, fire would not continue to burn, then he obtained a kind of air which was in all respects like ordinary air. The gas which sustained and improved the flame in the above experiment he called "fire-air"; the other gas, which did not support combustion, he called "vitiated air." The two gases were subsequently christened "oxygen" and "nitrogen" respectively.

Illustr. 158.—Scheele's Apparatus for Burning Hydrogen in Air

Hydrogen, generated in bottle A, immersed in hot water in BB, was ignited at the end of a glass tube. A flask C was placed over the flame, and the water rose to the level D, at which point the flame went out. One-fifth of the air had disappeared.

When Scheele heated saltpetre in a glass retort the bladder was distended by a gas which also turned out to be pure "fire-air" (O). Thereupon he repeated his previous experiments with liver of sulphur, phosphorus, etc., substituting "fire-air" for ordinary air. There now remained almost no residue, nearly all the gas was absorbed. But when he mixed some "vitiated air" with the "fire-air" and introduced a piece of phosphorus into this mixture of gases, then only the "fire-air" was absorbed. All these experiments showed that "fire-air" was the gas which supported fire in atmospheric air. "Fire-air," Scheele remarked, is mixed, in the case of atmospheric air, with another kind of gas which does not contribute anything

to combustion. This other gas merely hinders a too rapid and violent conflagration. Scheele prepared "fire-air" not only by heating saltpetre, or a mixture of "manganese" and sulphuric acid, but also by heating such oxides as oxide of gold, or red oxide of mercury, which last had been used also by Priestley.

Scheele's experiments with "manganese" threw light not only on oxygen but also on manganese, chlorine, and baryta or barium oxide (BaO), which last happened to be an impurity in the "manganese" which he used in his experiments. He also discovered the barium compounds, and noted the insolubility of the sulphate.

To Scheele's credit also falls the discovery of sulphuretted hydrogen, chlorine, hydrofluoric acid, baryta, prussic acid, molybdic acid, *tungstic acid, arsenic acid, manganates and permanganates, and* copper arsenite (a green arsenical pigment still known as "Scheele's green"). And he carried out experiments in which arsine (arseniuretted hydrogen), one of the most deadly of all poisonous gases, was produced.
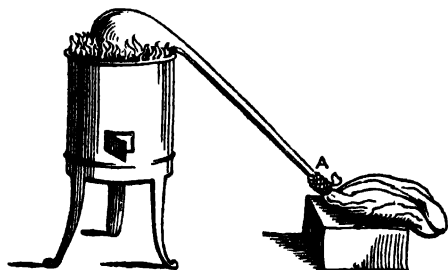
Illustr. 159.—Scheele's Apparatus for Collecting Gases

A bladder, pressed free of air, was tied to the end of the neck, A, of a retort, placed on a furnace, and contained the substance or substances from which the gas was to be produced.

Some of the discoveries in the chemistry of gases which are usually put to the credit of Priestley and others were known to Scheele, whose experimental researches embraced, besides oxygen, nitrogen, and carbon dioxide, also hydrochloric acid, sulphuretted hydrogen, and nitric oxide. He discovered, moreover, that the two components of air, which he named "fire-air" and "vitiated air," have very different degrees of solubility in water. Water has the peculiar property of partially separating the two components of air, inasmuch as it absorbs "fire-air" more readily. This "fire-air" is indispensable to animals which live in the water. Their vital processes depend upon their intake of "fire-air" and expiration of carbon dioxide. The expired gas is discharged into the atmosphere, and the water is thus enabled to dissolve more fire-air for the use of these animals. Such, in bare outline, were the main results to which Scheele was led by his experiments, which were for the most part merely qualitative, rather than quantitative, in character.

Scheele may be regarded as one of the founders of organic

chemistry, which hardly existed before his time as a branch of science. By adding a solution of lime or lead to the sour juices of plants he obtained precipitates which he recognized as the salts of certain acids. By decomposing these precipitates by means of sulphuric acid he succeeded in preparing various vegetable acids, such as tartaric acid, citric acid, malic acid, lactic acid, and oxalic acid. He also discovered tannic acid and benzoic acid; and he obtained "acid of sorrel" (oxalic acid) from rhubarb roots, and showed that this acid was chemically identical with "acid of sugar" prepared by the action of nitric acid on sugar. The examination of urinary calculi led to his discovery of uric acid.

The decomposition of Prussian blue by means of sulphuric acid *led him to discover hydrocyanic acid in 1782. His study of this acid was a model of its kind.* In his researches on fats and oils, he separated what he called "the sweet principle of oils" (glycerol) by evaporating the aqueous layer left after the digestion of various oils with litharge and water.

All these results were of fundamental importance for the work of later investigators. The value of Scheele's researches was not undermined by the fact that they were carried on under the influence of the phlogiston theory, which indeed they specially helped *to overthrow. This is peculiarly true of his demonstration* (i) that air consists of two different gases of which only one, namely, that which he called "fire-air" (oxygen), supports combustion and all processes which are analagous to combustion, (ii) that "fire-air" can be separated from ordinary air, and (iii) that ordinary air can be produced by mixing "fire-air" with "vitiated air" in the proportion of one to four.

Scheele was also one of the first to study the chemical effects of light. J. H. Schultze was the first to observe, in 1727, that precipitates containing silver are sensitive to light. Scheele experimented with pure chloride of silver, and showed that sunlight reduced it to silver. He also discovered that the various rays which compose white light have different effects upon silver salt. One of his experiments in this connection was briefly described by him as follows. "Place a glass prism before the window and permit the bent rays of the sun to fall upon the floor; in this coloured light place a piece of paper which is besprinkled with horn-silver: it will be observed that the horn-silver becomes black far sooner in the violent colour than in the other colours" (*Collected Papers*, Eng. trans., p. 131). These discoveries prepared the ground for photography. It is noteworthy that Boyle had observed the blackening of silver chloride, but ascribed it to the action of the air, instead of to the action of the light.

The foregoing account deals with but a part of Scheele's work. His researches were extraordinarily extensive in range of subjects, and his skill in experimentation was as remarkable as his versatility. Yet all this work was done under most unfavourable conditions, and in the course of a very short life.

The next important investigator into the chemistry of air was Cavendish.

CAVENDISH

The Honourable Henry Cavendish (1731–1810) was the son of Lord Charles Cavendish, brother of the third Duke of Devonshire. He had the reputation of being "the richest among the learned, and the most learned among the rich" of his generation. He devoted himself whole-heartedly to chemical and physical research. In 1766 he published an account of his "Experiments on Factitious Air." "By factitious air," he wrote, "I mean in general any kind of air which is contained in other bodies in an unelastic state, and is produced from thence by art." He described how he collected "fixed air" and "inflammable air" over water. By "fixed air," he explained, he meant "that particular species of factitious air which is separated from alkaline substances by solution in acids or by calcination; and to which Dr. Black has given that name in his treatise on quicklime." "Inflammable air" was his name for what was later renamed hydrogen. He obtained it by dissolving zinc or iron or tin in "diluted vitriolic acid or spirit of salt." He measured the density of both these airs, and the solubility of "fixed air" in water. He found that when the same weight of zinc was dissolved in either of the named acids, the same volume of "inflammable air" was generated. He concluded that this air had come from the metals (an inference which harmonized with the phlogiston theory, which he accepted). In his experiments with "fixed air" Cavendish introduced two new methods of importance. He dried the gas by passing it through pearl-ashes (potassium carbonate); and he stored it over mercury (instead of over water). He also momentarily collected "marine acid air" (hydrogen chloride), and noted its ready solubility in water.

The most famous work of Cavendish was begun in 1781, and described by him in the *Philosophical Transactions of the Royal Society*, in 1784 and 1785. He repeated the experiments of Priestley and Warltire on the formation of dew in a dry vessel when a mixture of common air and "inflammable air" was exploded by means of an electric spark. He found "that 423 measures of inflammable air are nearly sufficient to completely phlogisticate 1000 of common air; and that the bulk of the air remaining after the explosion is

then very little more than four-fifths of the common air employed; so that as common air cannot be reduced to a much less bulk than that by any method of phlogistication, we may safely conclude that when they are mixed in this proportion and exploded, almost all the inflammable air, and about one-fifth part of the common air, lose their elasticity, and are condensed into the dew which lines

Illustr. 160.—Cavendish's Apparatus for determining the Weight and Density of Hydrogen

A is a bottle filled with dilute sulphuric acid, B is a glass tube luted to the mouth of A, and C a glass cylinder luted to B with a small hole at the top. C is filled with coarsely powdered dry "pearl ashes" (pot-ash). The whole apparatus was weighed, the lute used for connecting A and B being weighed separately. A weighed quantity of zinc was then added to A, and A and B were luted together. Hydrogen was set free, dried in its passage through C, and allowed to escape into the air. The loss in weight was determined by re-weighing and allowing for the displacement of the air in the apparatus by hydrogen. From a previous experiment, the volume of hydrogen set free by this mass of zinc was known. The density of hydrogen was then calculated from these figures.

Cavendish used the same or a similar apparatus for measuring the weight of gas evolved in chemical re-actions. The bottle, A, contains acid and is connected, via tube B, with another wider tube, C, open at the top to allow the escape of gas and packed with "pearl ashes" or "filtering paper" to dry the issuing gas. The apparatus was weighed. Then a weighed amount of metal or carbonate was added to A. When the reaction was over, the whole apparatus was re-weighed, The weight of gas evolved by a given weight of metal or carbonate was then calculated.
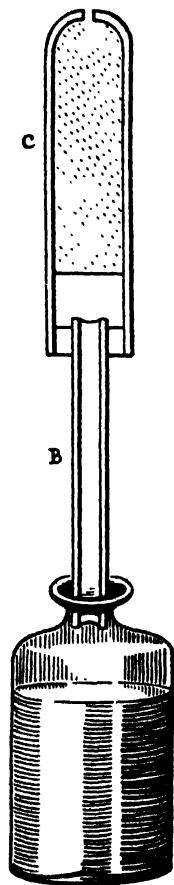
the glass." But what was this dew? To answer this question Cavendish repeated the experiment on a much larger scale, mixing and igniting gradually 500,000 grain measures of inflammable air with about 1,250,000 measures of common air. He obtained 135 grains of a liquid which on examination proved to be pure water.

Again he repeated the experiment with inflammable air, but using

"dephlogisticated air" instead of common air. Mixing the two gases in convenient quantities and exploding the mixture he used up 19,500 grain measures of "dephlogisticated air" and 37,000 of "inflammable air." The globe in which the explosions had taken place contained now 30 grains of liquid, which "was sensibly acid to the taste, and, by saturation with fixed alkali, and evaporation, yielded near two grains of nitre; so that it consisted of water united to a small quantity of nitrous [nitric] acid." Moreover, when the experiment was repeated with a greater excess of "dephlogisticated air," the liquid was more acid, whereas with an excess of "inflammable air," or with common air, there was no trace of acid

M
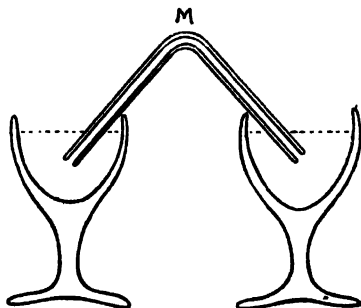
Illustr. 161.—Cavendish's Apparatus for Sparking Gases

M is a bent glass tube containing air over mercury and with its ends dipping into vessels containing mercury. Supplies of air, litmus solution, or soap-lees, were conveyed into M by means of a narrow glass tube, suitably bent so that its curved end engaged in one of the open ends of M and filled as required by inverting it, curved end uppermost, in receivers containing these substances, and releasing, by temporary removal of the finger from the other end, some of the mercury it was originally filled with. In subsequently conveying these materials into M, removal of the finger allowed the mercury pressure to force them in. (Compare Illustr. 155, Fig. 19.)

He concluded that "inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose, or else water united to phlogiston"; that "dephlogisticated air is in reality nothing but . . . water deprived of its phlogiston"; "that water consists of dephlogisticated air united to phlogiston";-and that the acid found in the experiment "proceeds only from the impurities mixed with the dephlogisticated and inflammable air."

Cavendish next turned to the study of "nitrous" [nitric] acid. Common air and some litmus solution were confined in a tube over mercury. The air was then sparked. It was found that the litmus had turned red, and that, "conformably to what was observed by Dr. Priestley," the air had diminished. He repeated the experiment,

substituting soap-lees for the litmus. He found that with "good dephlogisticated air" the diminution in the volume of air was small, and that with "perfectly phlogisticated air" no diminution occurred. "But when five parts of pure dephlogisticated air were mixed with three parts of common air, almost the whole of the air was made to disappear," and the solution produced in the soap-lees when dried by evaporation "left a small quantity of salt which was evidently nitre." He concluded that "nitrous acid" was a combination of "phlogisticated" with "dephlogisticated air." This incidentally accounted for the appearance of this acid in some of his previous experiments.

One feature connected with these experiments proved to be of special interest. Cavendish always found a small residue of air in these last-mentioned experiments. Repeated sparking of "phlogisticated air" with excess of "dephlogisticated air," and absorption of the excess by liver of sulphur, still left "a small bubble of air unabsorbed, which certainly was not more than $\frac{1}{120}$ of the bulk of the phlogisticated air let up into the tube; so that if there is any part of the phlogisticated air of our atmosphere which differs from the rest, and cannot be reduced to nitrous acid, we may safely conclude that it is not more than $\frac{1}{120}$ part of the whole." When in 1894 Rayleigh and Ramsay isolated this residual part, namely argon, they found that it forms 0·94 per cent of common air, as compared with Cavendish's remarkably near estimate of $\frac{1}{120} = 0 \cdot 83$ per cent.

(See J. R. Partington, *A Short History of Chemistry*, London, 1948, and *The Composition of Water*, London, 1928; and J. R. Partington and D. McKie, "Historical Studies on the Phlogiston Theory," in *Annals of Science*, 1937–39.)

CHEMISTRY (II)

C. The Chemical Researches of Lavoisier

THE experimental researches on combustion and respiration, on which chemists from Boyle and Hooke to Priestley, Scheele, and Cavendish had been engaged, came to a head in the researches of Lavoisier, whose interpretations of them first brought out their real significance. Antoine Laurent Lavoisier (1743–94) was born in Paris. His father was a wealthy man who was interested in science, and accordingly gave his son a good scientific education. Young Lavoisier showed mathematical ability, but his chief interest lay in chemistry, especially in applied chemistry. At the age of twenty-two he was awarded a special gold medal by the King for an essay submitted to the Academy of Sciences in a competition on the problem of the lighting of cities and large towns at night. In 1768 he was elected a member of the Academy of Sciences. Soon afterwards he was appointed a farmer-general of taxes. The income from this post was spent on his costly experiments. Later on he became Director of the nitre and gunpowder factories, a post for which he was eminently qualified by his knowledge of chemistry and his insight into practical affairs.

Lavoisier had learnt from Boyle's writings that lead or tin, when heated in a closed vessel containing air, will change into the corresponding metallic calx and gain in weight. But he determined to carry out an independent experimental investigation of the facts. He placed a weighed quantity of tin in a flask which he then sealed and heated until the tin was calcined. After the flask had cooled, it was weighed again with its sealed contents. The total weight had not changed. This disproved Boyle's suggestion that during calcination some of the fire particles penetrate into the retort and combine with the metal. Lavoisier next opened the flask, and observed that a quantity of air rushed into it, so that the flask now weighed more than when it was still sealed. He then weighed the calx of tin, and found that it had gained in weight just as much as the flask gained after the inrush of air when it was opened. The only possible explanation of these experimental results was that during their calcination metals combine with air, and gain in weight accordingly. Lavoisier reported these results to the Paris Academy of Sciences in 1774. At that time he was not yet acquainted with

Lavoisier

Berthollet

Illustr. 164

Baumé

Illustr. 165
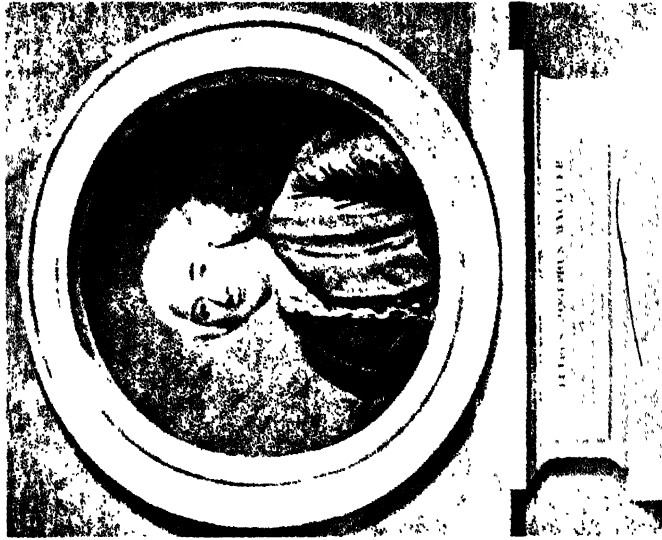
Macquer

the experiments which had shown that air is a mixture; he therefore could not pursue the problem to a more satisfactory solution. In the same year, however, Priestley visited Paris, and acquainted Lavoisier with his own experimental work, more particularly with his discovery of "dephlogisticated air" and its mode of preparation from red oxide of mercury. This gave Lavoisier the clue to the correct solution of his previous problem. For he showed soon afterwards that combustion, which is really like calcination, consists in the combination of the combustible substance with that part of the air which alone supports combustion—the part, namely, which Priestley and Scheele had respectively called "dephlogisticated air" and "fire air," and which Lavoisier at first described as "the purest part of air," "vital air," and finally as "oxygen" (i.e., generator of acids).

In 1773 Lavoisier repeated an experiment of Priestley's (*Phil. Trans.*, 1772, pp. 228–30) on the calcination of lead and tin by means of a burning lens in air confined over water or mercury. His experiments with tin were not successful, but with lead he
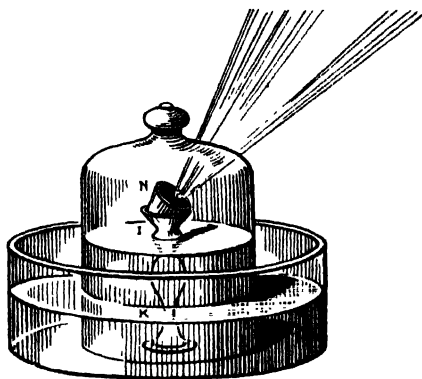
Illustr. 166.—Lavoisier's Experiments on the Calcination of Lead by means of a Burning Lens in enclosed air. The lead was placed in cup N, supported on stand IK over water or mercury in a jar.

found that there was a diminution of $\frac{1}{20}$ in the volume of the air, whereas Priestley had found a diminution of $\frac{1}{5}$. Lavoisier ascribed the diminution to "an absorption, a fixation of elastic fluid" by the metal on calcination (*Opuscules physiques et chimiques*, Paris, 1774; Eng. trans. by T. Henry, *Essays Physical and Chemical*, London, 1776, pp. 326 f.).

Lavoisier's experiments on combustion, made from 1775 onwards, are deserving of close attention, and will, therefore, be summarized now, from the account subsequently given in his *Traité Elémentaire de Chimie* (1789).

Lavoisier took a retort, about 36 cubic inches capacity, with a long neck. He bent this in such a way that the retort could be placed on a furnace (Illustr. 167) in such wise that the open end of its neck, E, could be in a bell-jar placed in a trough containing mercury, R. He put four ounces of pure mercury into a retort, and

by means of a syphon placed under the bell-jar, he raised the mercury to the level L. This level was carefully marked, and the barometric pressure and the temperature were duly noted. The fire in the furnace was then lighted, and the mercury was kept continuously near its boiling point for a period of twelve days. During the first day nothing remarkable happened. On the second day small red particles appeared on the surface of the mercury. They increased in number and in size until the seventh day. After that they ceased to increase, and continued unchanged. When the calcination of the mercury ceased to make any further progress the fire was allowed to go out and the vessels were allowed to cool. The total volume of air contained in the retort and in the bell-jar before the experiment amounted to 50 cubic inches, at a barometric pressure of 28 inches, and a temperature of 10° R. At the conclusion of the experiment the air had been diminished in volume to between 42 and 43 cubic inches, at the same temperature and barometric pressure. The air had, in other words, lost about one-sixth of its original volume. Lavoisier next collected carefully the red particles which had formed on the surface of the mercury, and removed from them as much as possible of the mercury adhering to them.

Illustr. 167.—Lavoisier's Apparatus for Experiments on Combustion

He weighed them; their weight was 45 grains. The residual five-sixths of the original volume of air left in the retort and bell-jar after the completion of the calcination was tested and found unsuited for supporting combustion or respiration. Animals placed in it died after a few moments, a lighted taper put in it went out immediately. Lavoisier next placed the 45 grains of calx in a small vessel connected with a receiver. When the retort was heated the calx yielded 41·5 grains of mercury and between 7 and 8 cubic inches of an elastic fluid that was much more potent for supporting combustion and respiration than is ordinary air. "This species of air," Lavoisier wrote, "was discovered almost at the same time by Mr. Priestley, Mr. Scheele, and myself. Mr. Priestley gave it the name of *dephlogisticated air*; Mr. Scheele called it *empyreal air*; at first I named it *highly respirable air*, for which the term *vital air* has since been substituted. We shall presently see what we ought to think of these denominations. In reflecting upon the circumstances of this experiment we readily perceive that the mercury, during its calcination, absorbs the salubrious and respirable part of the

air, or, to speak more strictly, the base of this respirable part; that the remaining air is a species of mephitis, incapable of supporting combustion or respiration; and consequently that atmospheric air is composed of two elastic fluids of different and opposite qualities" (*Elements of Chemistry*, tr. by R. Kerr, 1790, pp. 36 f.). He confirmed this discovery by means of the corresponding synthetic experiment. He mixed the two gases in the proportions found in his analytic experiment, namely 8 parts of oxygen to 42 parts of nitrogen, and obtained a gas which resembled atmospheric air in every respect, and, just like it, supported combustion, respiration, and the calcination of metals.

Earlier, in 1773, Lavoisier had heated red mercury calx together with carbon, and obtained "fixed air" instead of oxygen. He concluded subsequently that "fixed air" must be a compound of carbon and oxygen. This inference was confirmed by his previous experiments (1772) on the combustion of the diamond. A diamond, enclosed in a glass vessel containing air, was ignited by means of a powerful burning-lens, and the only product was "fixed air." Charcoal behaved in exactly the same way. Diamond thus appeared to be chemically rather like carbon. When a diamond was packed in charcoal powder and submitted to intense heat, it underwent no change. This showed that diamond by itself is not fusible, and does not become volatile through mere heating, but is only converted into a gas "fixed air" (or carbon dioxide) in the presence of oxygen.

In 1772 Lavoisier had carried out some experiments on phosphorus and sulphur, and discovered that these substances likewise gain in weight during calcination. Some years after obtaining the results described above, it naturally occurred to him that this increase in weight may have been due to their combination with oxygen. To verify this, he ignited by means of a burning-lens a weighed amount of phosphorus in a weighed bottle enclosed in a bell-jar containing air over mercury. When the combustion ceased, he replaced the stopper of the bottle, re-weighed it, and found an increase in weight. These results were first published by Lavoisier in his *Opuscules Physiques et Chimiques*, 1774 (Eng. trans. by T. Henry, *Essays Physical and Chemical*, 1776, pp. 383–6).

It will have been observed that Lavoisier strove in his experiments to deal with every chemical process at once qualitatively and quantitatively, though the results which he obtained frequently differed considerably from the more exact results obtained by others even in his own time. The qualitative aspect of the experiment just cited was described by him, in his *Traité Elémentaire de Chimie* (1789), in the following terms:

"The combustion of phosphorus succeeds equally well in atmospheric air and in oxygen gas, with this difference that the combustion is vastly slower, being retarded by the large proportion of azotic gas mixed with the oxygen gas, and that only about one-fifth part of the air employed is absorbed, because as the oxygen gas only is absorbed the proportion of the azotic gas becomes so great towards the close of the experiment as to put an end to the combustion. I have already shown that phosphorus is changed by combustion into an extremely light, white, flakey matter; and its properties are likewise entirely altered by this transformation. From being insoluble in water it becomes not only soluble but so greedy of moisture as to attract the humidity of the air with astonishing rapidity. By this means it is converted into a liquid, considerably more dense and of greater specific gravity than water. In the state of phosphorus before combustion it had scarcely any sensible taste, by its union with oxygen it acquires an extremely sharp and sour taste; in a word, from one of the class of combustible bodies it is changed into an incombustible substance, and becomes one of those bodies called acids" (*op. cit.*, Eng. trans., pp. 60 f.).

Lavoisier substituted the name "oxygen" for "pure air," etc., when he discovered that the combination of phosphorus or of sulphur with oxygen produces phosphoric or sulphuric acid respectively, and that the former changes into sulphuric acid upon further oxidation. This view of oxygen as a generator of acids needed correction in the light of the subsequent discovery that such acids as hydrochloric acid and hydrocyanic acid are devoid of oxygen.

Lavoisier furnished the correct explanation of respiration as well as of combustion. Respiration, according to him, consists in the combination of oxygen with the constituents of organic matter. Like combustion it sets free a quantity of heat. Carbon dioxide, which is the most essential product of respiration, derives its carbon from the organism and its oxygen from the atmosphere. The analogous character of respiration and combustion was further confirmed by the fact that Lavoisier obtained carbon dioxide and water by merely burning organic substances like alcohol, sugar, oil, and wax. Lavoisier also determined the amount of carbon and hydrogen contained in the burned organic substances from the quantity of carbon dioxide and water produced. In virtue of such determinations he may be regarded as the founder of organic analysis. He attempted to ascertain the percentage composition, by weight, of the substances which he examined. Thus, for instance, he determined the quantitative composition of carbon dioxide by oxidizing a weighed amount of carbon by means of red lead. From the loss of weight which the red lead suffered in the process he estimated

that carbon dioxide contains 72·1 per cent of oxygen—a good approximation to the correct value (72·7 per cent).

Another chemical problem solved by Lavoisier related to the nature of water. In 1781 Cavendish, as has already been pointed out, had shown that the combination of hydrogen and oxygen yielded water, and nothing else. Lavoisier continued this investigation by the analytical method. Cavendish began his work on water in 1781, and the results were read before the Royal Society in January 1784, and published in the *Philosophical Transactions* for 1784, with an interpolation by Sir Charles Blagden, who had been elected secretary of the Royal Society in May 1784. Blagden had been on a visit to Paris in May or June 1783, and in conversation had informed Lavoisier of the results obtained by Cavendish. Lavoisier seems to have thought that Cavendish's conclusions were unwarranted, and with Blagden he repeated the experiment in a rather crude way; but he promptly communicated his results to the Academy of Sciences on the very next day. And when the *Mémoires* were published (they were often several years in arrears), this contribution appeared in the volume for 1781, Lavoisier claiming the discovery as his own. In the interpolation in Cavendish's memoir of 1784, Blagden stated that he had informed Lavoisier of Cavendish's work as just explained.

Lavoisier read his paper before the Academy in 1783, and added to it before it was printed in the volume for 1781. Briefly, Lavoisier's experiment consisted in burning a mixture of oxygen and hydrogen, at a jet, over mercury in a bell-jar. The gases were conveyed to the jet by means of two leather tubes, one from a vessel containing oxygen and the other from another vessel containing hydrogen. Water formed on the sides of the jar; it was collected, and proved to be pure. The quantities of the gases were not observed; the water weighed just under 5 drachms.

From this experiment, Lavoisier went so far as to conclude that the weight of water formed was equal to the weights of the component gases, "inflammable air" and "vital air," and that water was therefore not a simple substance, but a compound of the two "airs."

In 1783 Lavoisier and Meusnier carried out further experiments on the composition of water; but the published account of this work also contains results obtained later on. They first found that iron filings slowly set free "inflammable air" from distilled water. Then they decomposed water by allowing it to drip, via a funnel, through a sloping iron gun-barrel, heated to redness in a furnace. A tube attached to the other end of the barrel led gaseous products to a suitably placed receiver. As the iron tube was extensively corroded

and its internal diameter became much narrower in consequence, they substituted a thick copper tube packed with small pieces of iron. The iron was oxidized and a considerable quantity of "inflammable air" collected. The quantitative result was of a low order of accuracy, but in its qualitative aspect it provided analytical confirmation of Cavendish's synthetical result.

Before concluding this account of the chemical researches of Lavoisier, reference may be made to one of his earliest investigations which gave the *coup de grâce* to a very old error. From ancient times it was believed by many people that water could change into earth. Early explanations of the formation of deltas, and of the displacement of water by land, were usually based on this belief; some of the experiments of Van Helmont, Boyle, and others appeared to support it; and it seemed to be a matter of common observation that even distilled water left an earthy residue after evaporation. Lavoisier attacked the problem experimentally, and reported his results to the Paris Academy of Sciences in 1770. He took a distilling vessel, then known as a "pelican," weighed it when empty, and also when containing some repeatedly distilled rainwater. The vessel was heated for a while to allow some of the air to escape, and was then firmly stoppered. It was heated on a sandbath from October 26, 1768, until February 1, 1769. Solid particles first appeared in the water about December 20th, and their number slowly increased. When cool, the vessel was weighed, the water meanwhile having been transferred to another container. The vessel had lost about 17 grains (actually 17·38) in weight. Lavoisier concluded that this lost matter accounted for the appearance of the solid particles in the water. To confirm this, the water was evaporated and the earthy residue weighed. Its weight was about 20 grains (actually 20·40). This difference he explained as due to the conditions of the experiment, and probably caused by further solution of earthy matter from the second vessel in which the water had been temporarily placed when removed from the first. Thus the earthy residue given by distilled water on evaporation was due, not to material transformation of water into earth, but to the solvent action of the water on the containing vessel.

It is interesting to note that Scheele in the preface to his *Chemical Treatise on Air and Fire* (1777) arrived at the same conclusion, though on merely qualitative evidence. He boiled distilled snow-water in a glass flask for twelve days. It became turbid. When cool, the water was passed off from the solid matter which had subsided. This water possessed alkaline properties; and the earthy residue behaved like silica "mixed with very little lime" (Dobbin's translation, pp. 88 f.). Moreover, the inside surface of the flask was "dim

and without lustre" up to the level where the water had stood in it. Whence Scheele concluded that the water had decomposed some of the glass and thus produced an earthy residue. "I accept it as certain," he wrote, "that neither by art nor by nature can pure water, by itself, be converted into a dry material which has all the properties of a true earth" (*Ibid.*, p. 88).

By 1785, or thereabouts, the phlogiston theory was beginning to decline. The new chemistry was gaining ground under the leadership of Lavoisier, and received its first comprehensive expression in his *Traité Elémentaire de Chimie* (1789). But unfortunately Lavoisier did not live to witness the growing acceptance and final triumph of the new movement. His classical text-book on chemistry proved to be his tombstone as well as his monument. Its publication coincided with the outbreak of the French Revolution; and, although the National Assembly made some use of his services, the Reign of Terror which followed had "no need of men of science." His official position under the monarchy was neither forgotten nor forgiven. He was tried for having added more water to the tobacco than was permitted during the period in which he controlled the monopoly of it. It was a trumpery charge, but he was condemned to death, and executed on May 8, 1794. Irreligion showed that it could be as cruel and fanatical as religion, but it could as little stem the steady advance of science.

Lavoisier's methods and views exercised a potent influence, and helped to bring chemical science to something like a position of equal rank with physics. This was largely due to the extension of exact quantitative methods, which chemistry owes to him and to his predecessor, Black. One result of the use of quantitative methods in chemistry was the clearer emergence of the principle of conservation of matter. For quantitative chemistry would be impossible without the postulate that matter is neither created nor annihilated, but remains constant in quantity, throughout all processes of chemical change. Moreover, this habit of quantitative precision also encouraged the tendency to give precision to the concepts used. So Lavoisier took up Boyle's definite conception of a chemical element, namely, as a homogeneous substance incapable of decomposition into simpler components, and applied it more fruitfully than Boyle ever succeeded in doing. He identified as elements oxygen, hydrogen, nitrogen, carbon, sulphur, phosphorus, and the metals. Lavoisier also showed his genius in declining to regard the alkalis, potash, and soda as elements, although he was unable to analyse them. For he strongly suspected them of being rather like the calces of metals, or compounds of oxygen with unknown metals. And his suspicions were subsequently confirmed when electro-

chemical methods were applied in the examination of these substances.

(See M. Berthelot, *La Revolution Chimique—Lavoisier*, Paris, 1890; D. McKie, *Antoine Lavoisier*, 1935; A. N. Meldrum, *The Eighteenth-Century Revolution in Science*, Calcutta, 1929; J. R. Partington, *The Composition of Water*, London, 1928.)

MONGE

While Cavendish and Lavoisier were studying the problem of the composition of water, Gaspard Monge (1764–1818) also worked
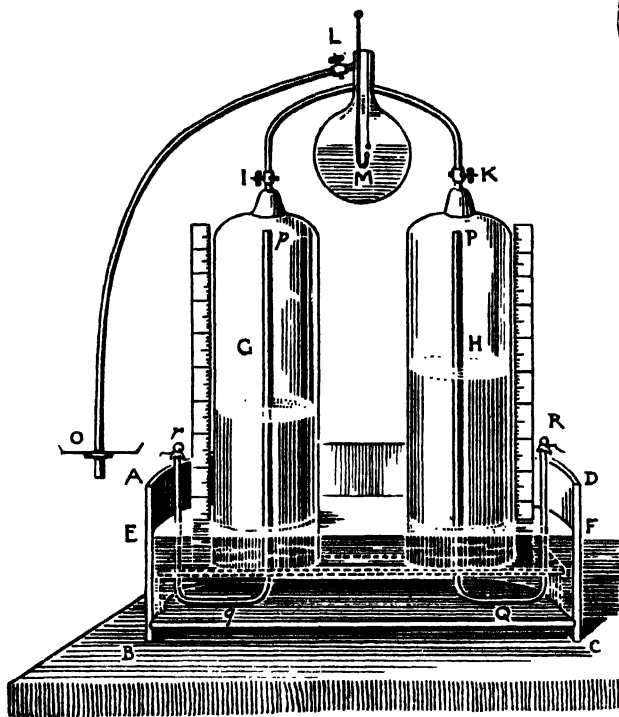
Illustr. 168.—Monge's Apparatus for the Synthesis of Water

ABCD is a pneumatic trough containing water. G and H are graduated glass cylinders. RQP and *rqp* are delivery tubes for admitting oxygen and hydrogen. Measured quantities of the gases are taken through taps I and K into the glass vessel M, previously exhausted by the pump O through the tap L, and there exploded by an electric spark.

quantitatively, following a method resembling that of Cavendish. Monge carried out his experiments in 1783, and an account of them appeared in the *Mémoires* of the Paris Academy of Sciences for 1783

(published in 1786). This account is of some interest because, unlike Cavendish, Monge gave diagrams of his apparatus (see Illustr. 168). Monge did not derive his method from Cavendish, but rather from Volta's method of exploding gaseous mixtures by electric sparks. Briefly, his method was as follows: Measured volumes of oxygen and hydrogen were conveyed into an evacuated glass globe and there exploded, the water produced being collected and weighed. In 372 explosions, Monge used $145\frac{91}{144}$ pints of "inflammable air" and $74\frac{9}{16}$ pints of "dephlogisticated air," and he obtained 7 pints of residual air and 3 ounces, 2 drachms, $45\cdot1$ grains of water. Allowing for the weight of the residual air (2 drachms, $27\cdot91$ grains), and calculating, from density determinations, the weights of "inflammable air" and "dephlogisticated air" originally taken, Monge found that the total air used was 3 ounces, 6 drachms, $27\cdot56$ grains, and that the total weight of the products was 3 ounces, 5 drachms, $1\cdot01$ grains—a discrepancy of 1 drachm, $26\cdot55$ grains, which he ascribed to various named experimental errors. He found that the water produced was slightly acid, and he attributed this to the possible presence of sulphuric acid, since he had prepared his "inflammable air" by the action of this acid on iron.

Monge concluded from his results that the explosion of pure "inflammable air" with pure "dephlogisticated air" gave pure water, heat, and light. His quantitative results were not so accurate as those obtained by Cavendish, since his density determinations were seriously affected, particularly in the case of hydrogen, by the fact that he had not dried his gases.

D. CHEMICAL AFFINITY AND EQUIVALENTS

Lavoisier's opposition to the phlogiston theory at first found more favour among eminent physicists and mathematicians like Laplace than among chemists. Most chemists considered the new movement too revolutionary. The first chemist of distinction to adopt Lavoisier's theory was Black. He was followed by Berthollet, whose researches on chemical affinity were of great importance in the subsequent development of chemistry.

BERTHOLLET, ETC.

Claude Louis Berthollet (1748–1822) was born in Savoy, studied medicine, and was appointed physician to the Duke of Orleans in 1772. This post afforded him ample leisure to pursue chemical researches, which at first related to the constitution of atmospheric air. He was elected a member of the Paris Academy of Sciences in 1780, and soon afterwards the French Government appointed him

to the post of technical director of the dye-works. He introduced many improvements into this industry, including the use of chlorine for bleaching. When, after the outbreak of the Revolution, France was isolated, and all foreign supplies were cut off, Berthollet rendered great services to his country as a technical chemist. He helped to develop her internal resources, more especially by reorganizing, improving, and extending the manufacture of steel and saltpetre. In 1792 he was appointed Director of the Mint, and shortly afterwards he was appointed member of a commission for promoting the prosperity of France by developing her agriculture and industries. About the same time he was elected Professor of Chemistry in Paris. It was only by making himself so useful, practically indispensable, that Berthollet escaped the tragic fate of Lavoisier in that period of storm and stress.

Berthollet discovered experimentally the chemical nature of ammonia (1786), hydrocyanic acid (1787), and sulphuretted hydrogen (1796). These experiments were of great importance. Priestley had shown that ammonia gas increased in volume as the result of electrical discharges. Berthollet discovered that the volume was just doubled, and that during this process the ammonia was decomposed into approximately three parts of hydrogen and one part of nitrogen. Berthollet also prepared the ground for Gay-Lussac's discoveries relating to the cyanides, by showing that hydrogen cyanide (HCN) was composed of carbon, nitrogen, and hydrogen only. Furthermore, he showed that sulphuretted hydrogen (H_2S), although it did not contain oxygen, had all the properties of an acid. His researches into chlorine led him, moreover, to the discovery of potassium chlorate ($KClO_3$), in 1788.

In 1774 Scheele had discovered the gas chlorine and observed its bleaching properties. Berthollet in 1785 prepared chlorine-water by passing chlorine into water, and found that this solution had bleaching properties. In 1788 he found it more convenient to pass the chlorine into solutions of caustic potash, whereby a solution of hypochlorite was formed; and this rapidly became of industrial importance in the bleaching industry under the name of *Eau de Javelles*. Berthollet's work in this field laid the foundations of the bleaching industry, and his industrial successes by his application of the new doctrines of Lavoisier, and his use of the new nomenclature, did much to establish the new system of chemistry.

In 1785 Berthollet discovered that nitrous oxide could be prepared by heating ammonium nitrate.

Berthollet's services to chemistry were not confined to its experimental or technical side. He made equally valuable contributions to chemical theory. In this department of chemistry his fame rests

chiefly on his comprehensive researches into the nature of chemical affinity, about which something must be said here.

Ideas of "hostility" between some substances and "elective affinity" between others, were current long before this period. Boyle, for instance, had expressed his dissatisfaction with the "supposed hostility between . . . acids and . . . alkalis," and showed that salts were formed by the combination of an acid and an alkali, and that one acid or base could replace another acid or base in a salt (*Works*, ed. by T. Birch, 1772, Vol. IV, p. 289, and Vol. I, p. 359). Various other chemists took up the study of salts. These studies were largely influenced, not only by the chemical ideas of Boyle, but also by Newton's ideas of attractive forces between bodies, and gave rise to various tables of elective affinity between substances. Among the earliest of such tables were those of E. F. Geoffroy (1672–1731), who, in 1718, tried to show the order of affinity of a base for various acids, or of an acid for various bases. He proceeded on the assumption that if one acid had a greater affinity for a certain base than another acid had, then the former acid would displace the latter from a salt formed by its combination with that base. Geoffroy, accordingly, drew up tables of similar substances arranged according to their power to displace one another in combinations with the substances named at the head of the tables (*Mém. de l'Acad. Roy. des Sciences*, 1718, p. 202). It was soon found, however, that the affinity of one substance for another was not constant. A. Baumé (1728–1804), more particularly, showed, in 1773, that these affinities varied according as the reactions were carried out in solutions at ordinary temperatures ("the wet way"), or by heating the substances together at higher temperatures ("the dry way"). Different tables of affinity were consequently required for the two "ways" or conditions of reaction (*Mém. de Math. et de Phys. présentés à l'Acad. Roy. des Sciences . . .*, Paris, 1774, Vol. VI, pp. 231–6).

T. O. Bergman (1735–84), Professor of Chemistry at Upsala, set to work, during the years 1775–83, to prepare such tables of affinity as Baumé had pronounced to be necessary. With enormous labour Bergman studied a wide range of substances, and drew up two tables of affinity for each of fifty-nine different substances. The results were published in their final form in Bergman's *Opuscula Physica et Chimica*, Upsala, 1783, Vol. III (English version, *Dissertation on Elective Attractions*, London, 1785, by "the Translator of Spallanzani's Dissertations"). Unfortunately Bergman did not realize the importance of taking into account all the physical conditions attending chemical processes, but was prone to regard affinity as a constant that was little influenced by external conditions other than heat. "In this dissertation," he wrote, "I shall endeavour to determine the order

of attractions according to their respective force; but a more accurate measure of each, which might be expressed in numbers, and which would throw light on the whole of this doctrine, is as yet a desideratum" (Eng. trans., p. 4). He obtained his results according to the following principles: "Suppose A to be a substance for which other heterogeneous substances a, b, c, etc., have an attraction; suppose, further, A, combined with c to saturation (this union I shall call Ac), should, upon the addition of b, tend to unite with it to the exclusion of c, A is then said to attract b more strongly than c, or to have a stronger elective attraction for it; lastly, let the union of Ab, on the addition of a, be broken, let b be rejected, and a chosen in its place, it will follow that a exceeds b in attractive power, and we shall have a series, a, b, c, in respect of efficacy. What I here call attraction, others denominate affinity; I shall employ both terms promiscuously in the sequel, though the latter, being more metaphorical, would seem less proper in philosophy" (*ibid.*, pp. 6 f.). The task which Bergman set himself involved more than 30,000 experiments, even apart from the further complications likely to be caused by the discovery of new substances with the continuous advance in chemistry. Yet, nothing daunted, Bergman pushed on with his colossal enterprise until ill health compelled him to realize that it was not for him to complete the task. So he published the results he had already obtained.

The work on chemical affinity was carried a stage further by Berthollet, who first showed that the affinity of substances was influenced by such factors as their mass, their solubility and volatility, or their insolubility and involatility, as the case might be. He insisted that "elective affinity, in general, does not act as a determinate force," and he urged that the study of chemical affinity must be put on a much wider basis. "A theory of chemical affinities," he wrote, "solidly established and serving as a basis for the explanation of all chemical questions ought to collect or contain all the principles from which the causes of chemical phenomena can proceed in every possible variety of circumstances; because observation has proved that all these phenomena are only the various effects of that affinity, to which all the various chemical powers of bodies may be attributed" (*Researches into the Laws of Chemical Affinity*, 1801; Eng. trans. by M. Farrell, 1804, pp. 1–4). Berthollet was led to lay stress on the *mass* of substances by his tendency to identify affinity with gravitation or "astronomical attraction," in which mass is, of course, a very important factor. He thought that the peculiarities of chemical affinity were due to the fact that gravitational attraction acted differently on bodies in close contact than on bodies far apart. In the former case it was influenced by the form and especially by the

close contact of the parts, their relation to the solvents, and their volatility. Berthollet is particularly interesting when he explains the influence of volatility. "When a substance assumes the state of a gas, on separating from an intimate combination it becomes elastic and can offer no further resistance to the decomposing action: whence it appears that substances of this nature do not act by their mass. The decomposing substance can then effect a complete decomposition; and it will suffice to employ just as much of it as would have been necessary to form the same combination immediately, or at least a very trifling excess. Thus carbonic acid may be disengaged from its combination by another substance whose affinity for the base of the carbonate might be less, because that other substance can act by its mass, and can therefore overcome the affinity of the carbonic acid, by acting successively; but to expel the whole of the carbonic acid, the decomposing substance must be used in somewhat greater quantity than is necessary to produce saturation" (*ibid.* pp. 46 f.).

Berthollet's conception of affinity tended to upset the then current classification of affinities, which was based on the assumption that one acid excluded another through the force of affinity. This tendency was strengthened when Berthollet showed that the solubility or insolubility (or "cohesion") of the resulting compound constituted an important factor in chemical changes. "Whenever," wrote Berthollet, "a body has a strong tendency to assume the solid state by combining with another in certain proportions, that tendency alone suffices to cause its separation in that state, independently of the force of elective affinity" (*ibid.*, p. 44).

In intimate connection with his work on affinities, and their dependence on various physical properties, Berthollet showed that chemical actions were reversible, and that if a reactant were present in large amount, its excess might compensate for its weakness of affinity. "In all the compositions and decompositions produced by elective affinity there takes place a partition of the base or subject of the combination, between the two bodies whose actions are opposed; and the proportions of this partition are determined, not solely by the difference of energy in the affinities, but also by the difference of the quantities of the bodies; so that an excess of quantity of the body whose affinity is the weaker compensates for the weakness of affinity" (*ibid.*, pp. 4 f.). He took the case of baryta and potassium. The interaction of these yielded caustic potash and barium sulphate, and the change was supposed to have been complete. But he showed that the reaction could be reversed, that caustic potash and barium sulphate could react to give potassium sulphate. Similarly, it used to be supposed that potassium carbonate was completely causticized by lime; but Berthollet showed that the action was reversible, for

he obtained potassium carbonate by the interaction of potash and calcium carbonate. "It is evident," he wrote, "that the bases which are supposed to form the strongest combinations with the acids may be separated from them by others, whose affinities are supposed to be weaker, and the acid divides itself between the two bases. It also appears that acids may be partially separated from their bases by other acids, whose affinities were supposed to be weaker; in which case the base is divided between the two acids" (*ibid.*, p. 11).

RICHTER

Jeremiah Benjamin Richter (1762–1807) carried out important quantitative investigations in chemistry. Little is known about Richter except that he was born at Hirschberg in Silesia, and that he was employed as a chemist in the Breslau mines, and later in the porcelain factory at Berlin. He invented the term *Stoicheiometry* for the branch of chemistry specially concerned with the quantitative chemical relations between reacting substances. He investigated particularly the reacting proportions between acids and bases, and although he was obsessed with the fantastic notion that the weights of the bases formed an arithmetical, and the weights of the acids a geometrical, progression, he discovered one of the fundamental laws of chemistry.

Before Richter there had been some realization of this problem. Cavendish (*Phil. Trans.*, 1767, p. 102) had spoken of the weights of "fixed alkali" (potash) and "calcareous earth" (lime) that saturated the same weight of a given acid, as equivalent to one another, and had observed (*Phil. Trans.*, 1788, p. 178) that the same *weights* of nitric and sulphuric acids that saturated equal weights of potash also decomposed equal weights of marble. Bergman noted that when one metal was precipitated by another from a neutral solution of one of its salts, the resulting solution was still neutral; and Lavoisier urged that cases of double decomposition should be investigated quantitatively to find out whether, in this exchange of bases between two acids, there appeared to be any excess of acid or not.

Richter published his researches in *Anfängsgründe der Stöchyometrie oder Messkunst chemischer Elemente* (1792–94) and in *Ueber die neueren Gegenstände der Chemie* (1791–1802). He proceeded from his discovery that two neutral salts on double decomposition yield neutral compounds—the so-called law of neutrality.[1] From this he concluded

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[1] Karl Friedrich Wenzel (1740–93), in his *Lehre von der Verwandtschaft der Körper* (Dresden, 1777), published certain studies on this problem which have been misinterpreted. For instance, in the reaction between copper sulphate and lead acetate, yielding lead sulphate and copper acetate, Wenzel found that neutrality was not maintained; that the acetic acid from the lead acetate was

that there must be fixed quantitative relations between the con-
stituents of these salts. "When two neutral solutions are mixed,"
he wrote, "and a decomposition follows, the new resulting products
are almost without exception neutral also"; and later, "The
elements must, therefore, have amongst themselves a certain fixed
proportion of mass" (*Stöchyometrie*, 1, 24; this section is translated in
R. Angus Smith's *Memoir of John Dalton*, etc., 1856, p. 190). Richter
wrote in an obscure style and tried to express his ideas mathe-
matically; sometimes he wrote in terms of the phlogiston theory,
and at others in terms of the oxygen theory; but he realized clearly
that, if he knew the combining ratios of acid and base in the original
compounds, then he knew also the ratios in the resulting compounds.
He therefore determined the amounts of the various acids and bases
that neutralized one another, *giving separate tables for each acid and
each base*, although he evidently realized that the various weights of
the bases which neutralized a fixed weight of one acid also neutralized
another fixed weight of a second acid, and he used this principle to
check some of his results. Further, he affirmed that the weights of
the alkalis, or alkaline earths, that neutralized a fixed weight of
sulphuric, hydrochloric, or nitric acid always stood in a fixed ratio;
and his figures for these may be expressed as shown on page 382 (see
I. Freund's *The Study of Chemical Composition*, Cambridge, 1904, p. 175).
Richter's figures are far from correct, and he has been charged
with altering them to fit his theory; but what is important here is
that he clearly saw that there should be a fixed ratio between the
amounts of the bases as shown in this table. Moreover, since the
law of neutrality had been observed by Bergman to hold also in
the case of a metal precipitated by another metal from a neutral
solution of one of its salts, Richter showed that by determining the
amounts in which metals precipitate one another from solutions of
their salts it was possible to calculate the proportions of oxygen in
their oxides.

Despite Richter's obscurity of style, he had shown quite clearly
that the weights of two substances that are equivalent in one
chemical reaction are equivalent in other chemical reactions. It
remained for Fischer to weld these data into one comprehensive
scheme.

insufficient to react with the copper from the copper sulphate; and that of one
hundred and twenty-four parts of copper, nine and a half remained undissolved.
Yet Berzelius later (in 1819) credited Wenzel with the discovery of the law of
neutrality—although Wenzel had actually believed the opposite to be the case—
and this mistake remained uncorrected until after 1850. It is noteworthy, however,
that in this research Wenzel discovered that the rate of chemical reaction was
proportional to the concentration of the reacting substance, a principle which
later on played a great part in chemical dynamics.

	1000 Sulphuric Acid	1000 Hydrochloric Acid	1000 Nitric Acid
Potash	1606	2239	1143
	$\dfrac{1606}{1218} = 1\cdot318$	$\dfrac{2239}{1699} = 1\cdot318$	$\dfrac{1143}{867} = 1\cdot318$
Soda	1218	1699	867
	$\dfrac{1218}{638}\quad 1\cdot909$	$\dfrac{1699}{889}\quad 1\cdot911$	$\dfrac{867}{453} = 1\cdot914$
Volatile Alkali ..	638		453
	$\dfrac{638}{2224}\quad 0\cdot287$	$\dfrac{889}{3099} = 0\cdot287$	$\dfrac{453}{1581} = 0\cdot287$
Baryta	2224	3099	1581
	$\dfrac{2224}{796}\quad 2\cdot795$	$\dfrac{3099}{1107} = 2\cdot800$	$\dfrac{1581}{565} = 2\cdot799$
Lime	796	1107	565
	$\dfrac{796}{616} = 1\cdot292$	$\dfrac{1107}{858} = 1\cdot290$	
Magnesia ..	616	858	438
	$\dfrac{616}{526} = 1\cdot171$	$\dfrac{858}{734} = 1\cdot169$	
Alumina	526	734	374

FISCHER

E. G. Fischer, when translating C. L. Berthollet's *Recherches sur les Lois de l'Affinité* into German (*Über die Gesetze der Verwandtschaft in der Chemie*, Berlin, 1802), combined Richter's tables into one table. Fischer wrote: "Richter has taken the trouble to examine each acid in its relations to the bases, both by experiment and by calculation, and to give his results in the form of tables. It seems that he paid no attention to the fact that all his tables could be reduced to a single one containing twenty-one numbers divided into two columns. I give the one which I have calculated from his newest data:

Bases		Acids		
Alumina ..	525	Hydrofluoric Acid ..		427
Magnesia ..	615	Carbonic ,, ..		577
Ammonia ..	672	Sebacic ,, ..		706
Lime ..	793	Muriatic ,, ..		712

Bases			Acids			
Soda	..	859	Oxalic	Acid	..	755
Strontia	..	1329	Phosphoric	,,	..	979
Potash	..	1605	Formic	,,	..	988
Baryta	..	2222	Sulphuric	,,	..	1000
			Succinic	,,	..	1209
			Nitric	,,	..	1405
			Acetic	,,	..	1480
			Citric	,,	..	1583
			Tartaric	,,	..	1694

"The meaning of this table is that if a substance is taken from one of the two columns, say potash from the first, to which corresponds the number 1605, the numbers in the other column indicate the quantity required of each acid to neutralize these 1605 parts of potash; there will in this case be required 427 parts of hydrofluoric acid, 577 parts of carbonic acid, etc. If a substance is taken from the second column, the first column will be used to ascertain how much of an earth or of an alkali is required to neutralize it."

Essentially, this is the first table of chemical equivalents, although they are not called such; and it embodies the Law of Reciprocal Proportions, although this name was not used till later. Richter had thus discovered, and correctly explained, the phenomenon of neutrality in double decompositions, had determined the equivalents of a number of acids and bases and (through Fischer's calculation) discovered one of the fundamental quantitative laws of chemistry. His results were accepted by Berthollet, whose early ideas on affinity as expressed in his *Researches on the Laws of Affinity* have already been discussed in this chapter; but it was through Berthollet's later book, *Essai de Statique Chimique* (1803), in which Richter's researches, hitherto ignored, were discussed, that his work became more generally known.

E. THE REFORM OF CHEMICAL NOMENCLATURE

Along with the reform in chemical theory, introduced by Lavoisier, came, almost necessarily, a reform in chemical nomenclature (system of names). There had already been some movements towards this. Bergman, Macquer, and Baumé had urged the need of a uniform system; and in 1782 Guyton de Morveau laid before the Paris Academy of Sciences a proposal for a new system of chemical nomenclature, which did not, however, meet with general approval because it was expressed in terms of the phlogiston theory, which was already in dispute. Presently, however, De Morveau accepted Lavoisier's views, and, in conjunction with Lavoisier, Berthollet, and Fourcroy, set about revising the whole nomenclature of the science.

In 1787 their system was published under the title *Méthode de Nomenclature Chimique*, Paris, 1787 (Eng. trans. by J. St. John, 1788). The reforms there introduced were subsequently incorporated by Lavoisier in his *Traité Elémentaire de Chimie*, which first appeared two years later, in 1789.

Starting from the position that a language is a kind of analytical apparatus necessary to the art of reasoning, they kept three objects in view: "The series of facts which constitute the science, the ideas which recall such facts, and the words that express them" (Eng. trans., p. 9).

"It must be supposed," wrote Lavoisier of their work, "that we could not have proceeded so far in these different matters without doing more or less violence to established custom, and without adopting designations which may sound harsh and barbarous at first; but we have remarked that the ear easily becomes accustomed to new words, especially when they are connected together in one general and rational system. At any rate the names which are in common use, such as *powder of algaroth, salt of alembroth, Pampholix, Phagadenic water, turbith mineral, Aethiops, Colcothar*, and several others, are neither less discordant, nor surely less extraordinary; continual practice and a good memory are necessary to retain ideas of the substances indicated by such terms, and especially to recollect to what genus of combination they belong. The terms *oil of tartar by the bell, oil of vitriol, butter of antimony, butter of arsenic, flowers of zinc*, etc., are still more ridiculous, because they give birth to false ideas; for, properly speaking, there does not exist in the mineral kingdom, and especially in the metallic, either butter, or oil, or flowers; in fine, because the substances expressed by these misleading names are for the most part violent poisons" (Eng. trans., pp. 16 f.).

These chemists, therefore, attempted to rationalize chemical names so that the name of a substance should express its chemical nature. Their work was so well conceived and carried out that the system which they introduced forms the basis of that in use to-day. As a first step, they divided all substances into two classes, namely, elements and compounds. The elements included all "the simple substances, that is to say, such as chymists to the present time have not been able to decompose" (Eng. trans., p. 21). The naming of these was considered important, "because the designations of bodies which by exact analysis can be reduced to their elements, are properly expressed by the reunion of the names of those same principles" (Eng. trans., p. 21). "Dephlogisticated air," which had already with good reason been renamed "vital air," now became "oxygen," as Lavoisier proposed, from the Greek words for "acid" and "beget," on account of the property of this principle, the basis of vital air, to

change a great many of the substances with which it unites into the state of acid, or rather because it appears to be a principle necessary to acidity (Eng. trans., p. 24). "Inflammable gas" became "hydrogen" from the Greek words for "water" and "beget"; experiments having proved that water is nothing but oxygenated hydrogen (Eng. trans., p. 24). "Phlogisticated air" became "azote," from the Greek for "without life," on account of its inability to maintain animal life (Eng. trans., p. 26). In 1790, however, Chaptal changed the name "azote" to "nitrogen," because he regarded it as a constituent of nitre. "Volatile spirit of sal ammoniac" had been renamed "ammonia" by Bergman in 1784, and continued to be known by this name.

Sulphur retained its name, and its acids were named thus: "Sulphur*ic* acid signifies sulphur as much as possible saturated with oxygen; which composition was formerly called vitriolic acid. Sulphur*eous* acid means sulphur united to a less quantity of oxygen; which before was called sulphureous vitriolic acid, or phlogisticated vitriolic acid" (Eng. trans., p. 29).

Further, "Sulph*ate* is the general name for all the salts formed by the sulphuric acid. Sulph*ite* signifies the salts formed by the sulphureous acid" (Eng. trans., pp. 29–30). "Sulphur*et* denotes all the combinations of sulphur not advanced to the state of acid, and regularly displaces the improper and absurd appellations of liver of sulphur, hepar, pyrite, etc." (Eng. trans., p. 30).

Another important change was explained as follows: "There is no substance whatever which has received so many different appellations as the gas which Dr. Black has called *fixed air*; at the same time expressly reserving the liberty of changing the name, which he confessed was improperly applied. The disagreement of the chymists of every country gives us, without doubt, a more perfect liberty, because it shows the necessity of motives being presented capable of making them all unanimous: and we have made use of that liberty according to our principles. As fixed air has been perceived to be produced by the direct combination of *charcoal* with vital air, by the assistance of combustion, the name of this gazeous acid can no longer be arbitrary, but necessarily must be derived from its radical, which is the pure carbonic matter; therefore it is *carbonic acid*,[1] and its compositions with different bases are *carbonates*; and, for the sake of greater precision in the designation of this radical, by distinguishing it from charcoal, according to the vulgar acceptation, to isolate it in thought from the small quantity of foreign matter which it generally contains, and which constitutes the ashes, we apply to it

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[1] It is worth noting that, already in 1773, Bergman had called it "aerial acid," on the ground that it had the properties of an acid.

the modified name of *carbon*, which indicates the pure and essential principle of charcoal, and which has the advantage of expressing it by a single word, so as to prevent all equivocation" (Eng. trans., p. 32).

"Marine acid" had already been re-named "muriatic acid," and, according to the view then held of its relation with the substance now known as chlorine, the latter had been called "dephlogisticated marine acid." They were now named respectively "muriatic acid" and "oxygenated muriatic acid." The salts of "muriatic acid" became "muriates," "butter of tin" thus becoming "muriate of tin," and so on. Although "phlogisticated air" was then called "azote," the terms "nitric acid" and "nitrous acid" were gradually adopted as derivations from "nitre."

The calces of metals by an obvious derivation became "oxyds" (Eng. trans., p. 40), and later "oxides."

Thus the binary compounds had names formed from their two constituent elements. In the case of the acids, the general name "acid" was qualified by a specific adjective indicating the element, other than oxygen, present in the combination—thus, sulphuric acid, carbonic acid, phosphoric acid, etc.—with distinguishing terminations, in cases where the element or radical forms two acids. In the case of the calces, the class designation "oxides" was followed by the specific name of the other element, e.g., oxide of lead, and so on.

Salts were named after the acid from which they were derived, e.g., sulphates; the name of the base being added, e.g., sulphate of zinc, to indicate the salt of a particular base.

"Fixed air," which, as has already been remarked, had been named "aerial acid" by Bergman in 1773 on account of its acid properties, was re-named "carbonic acid" in the new chemical nomenclature introduced in 1787.

In this way the whole nomenclature was revised, and the system thus introduced was subsequently expanded, without any essential change of principle, into the modern one.

(See E. von Meyer: *History of Chemistry*, 1891, etc.; T. M. Lowry: *Historical Introduction to Chemistry*, 1915; J. R. Partington: *A Short History of Chemistry*, 1948; and Sir P. J. Hartog: "The Newer Views of Priestley and Lavoisier," *Annals of Science*, 1941, Vol. 5, pp. 1–56.)

GEOLOGY

A. Geogony

Speculations on the origin and structure of the Earth continued to engage interest during the eighteenth century, as the writings of some of the English pioneers in this field became known in other countries. But, on the whole, there was as much criticism as imitation of the seventeenth-century hypotheses.

MORO

The Italian, Anton Lazzaro Moro (1687–1740), in his book *De' Crostacei e degli altri marini Corpi che si truovano su' Monti* (Venice, 1740), criticized the views of Burnet and Woodward, and put forward his own hypothesis. He contended that it was impossible to account for the presence of fossil shells on high mountains by reference to the Flood in the days of Noah. The phenomenon could only be explained by reference to volcanic action, such as had been exemplified by the eruptions of Etna and Vesuvius in ancient times, by the sudden rise of Monte Nuovo near Naples in 1538, and by the appearance of a new volcanic island in the Greek Archipelago as recently as 1707. Moro maintained that originally the Earth had a smooth, stony surface which was entirely covered with fresh water of no great depth. Subsequently, subterranean fires disrupted the Earth's surface, so that land and mountains rose above the level of the water, and all sorts of materials contained in the bosom of the Earth, such as clay, earth, sand, bitumen, salts, sulphur, etc., were discharged, and formed a new stratum above the Earth's original stony surface. The taste of sea-water is due to the salts and bitumen then discharged into the fresh water which originally covered the Earth. Through the recurrence of such eruptions, caused by subterranean fires, more land and mountains appeared, and more materials were discharged, forming new layers above the Earth's surface. As the new layers were not always laid down over the whole Earth at once, but at different times in the course of long periods, the kind of things entombed in them are naturally different. Moro, of course, did not deny the historicity of Noah's Flood. Rather, like his younger contemporary, Antonio Vallisneri (1661–1730), in his book *Dei Corpi marini che sui monti si trovano* (Venice, 1721), Moro maintained that a temporary Flood, like that in the days of Noah, could not account for the marine formations which extended over a considerable part

of Europe. The water must have covered the whole Earth for a very long time.

DE MAILLET

Benoît de Maillet (1656–1738) was a French diplomatist whose interest in geology appears to have been prompted by a contempt for human pretensions, which he confronted with the vision of a time when the Earth would dry up, and be burnt up by volcanic eruptions. His unorthodox views were put into the mouth of an Indian philosopher (Telliamed), but the book was not published till long after the author's death—*Telliamed ou Entretiens d'un Philosophe Indien avec un Missionaire Français* (Amsterdam, 1748). According to De Maillet the whole Earth is a marine deposit. Lands and mountains consist of marine sediments of sand, mud, etc. The highest and oldest mountains are simple and uniform in composition, and embody next to no traces of animal life. The subsidence of the sea exposed the tops of these mountains; and the attrition caused by the beating of the sea against them provided the material for new mountains and new strata, in which fossils are found in increasing abundance, and are arranged in the same order as similar organic remains are still found on the bed of the sea. This arrangement could not possibly have been brought about by such a local and temporary flood as the Deluge described in the Bible. The sinking in the water-level which exposed the mountain tops was due to evaporation which, he estimates, reduces the sea-level by about three feet in a thousand years. In course of time even the Atlantic will dry up, and eventually the whole Earth will flare up like a sun and consume all its combustible materials, after which it will cool down again into an earthy body. The conflagration referred to will be caused by large-scale volcanic eruptions, which, according to De Maillet, are simply the combustion of the fats and oils of organisms embedded in the sediments of which the Earth is made. As part of his geogony De Maillet propounded a kind of evolutionary view according to which all terrestrial plants and animals have evolved from corresponding marine organisms, with such changes in structure and function as the new habitat necessitated. His heterodoxy did not prevent De Maillet from indulging in such superstitious-beliefs as the existence of mermen and mermaids. He even suggested the precise locality—namely, the polar regions—where mermen and mermaids were transformed into mere men and women.

BUFFON

George Louis Leclerc, Comte de Buffon (1708–88) propounded in his *Théorie de la Terre* (1749, Eng. trans., as *Natural History*, by

William Smellie, Vols. I and IX, also in Vols. I and II of Barr's
Buffon, 1792), and in *Époques de la Nature* (1778), a view of the Earth
which exercised great influence upon eighteenth-century geology.
Like Descartes and Leibniz before him, and Kant and Laplace after
him, Buffon connected his account of the origin of the Earth with a
theory of the whole solar system. According to this theory the Earth
and the other planets were originally parts of the Sun, but were
broken off by the shock of a comet. In composition and motion they
consequently resemble the Sun. After their separation from the
parent body they were very hot and luminous, but gradually cooled
and darkened, whereas the Sun continued incandescent. The
abundance of fossil shells in all parts of the Earth convinced Buffon
that the sea must at one time have covered it all, and that the
appearance of dry land must have been due to the fracture of the
Earth's crust and the consequent disappearance of great masses
of water into the abysses and caverns thus formed.

In his later work (*Époques*) he attempted to distinguish seven
epochs in the history of the Earth.

(1) During the first period the Earth was still a molten mass, like
the parent Sun, and assumed its oblate spheroidal shape as the
mechanical result of its rotation. Gradually its outer surface cooled
and solidified, and eventually consolidated to the centre.

(2) This consolidation marked the second epoch, during which,
owing to the continued cooling of its mass, hollows were formed
in its interior and depressions and ridges on its surface, thus con-
stituting the oldest valleys and mountains. Except for the atmospheric
vapour round it, there was no water on the Earth till the end of this
epoch.

(3) The third period commenced when the Earth had cooled
sufficiently for the surrounding vapour to condense on its surface
and form a universal sea. Judging from the heights at which marine
fossils could be found, Buffon estimated the original sea-level at
from nine thousand to twelve thousand feet above the present
sea-level. Originally, however, the sea was too hot for animal life,
which only emerged subsequently when the waters had cooled.
The earliest organisms must have been very unlike their successors;
and the history of successive species might be determined from a
study of a suitable collection of fossils obtained from the highest
mountains. The water corroding the crust of the Earth produced a
sediment of clay; the calcareous fossiliferous deposits resulted from
the rapid increase in living organisms, with which the sea teemed.

(4) The fourth epoch commenced when sufficient water dis-
appeared through the rents of the cooling Earth to expose the lower
portions of the Earth's crust. About one per cent of the land became

covered with vegetation, a great part of which was swept down to lower levels, including crevices in the crust of the Earth, where it served as fuel for the volcanoes, which were soon to appear. Buffon regarded volcanic eruptions as the effect of subterranean electricity acting on combustible stones in the vicinity of the sea; and he credited such volcanic conflicts between fire and water with the formation of the great valleys.

(5) The fifth period was a period of calm which followed the volcanic fourth period. Land animals, such as the elephant, the hippopotamus, and the rhinoceros, now appeared in the warm regions, which then extended from Asia to Europe and America.

(6) The sixth epoch witnessed the separation of the continents of the Old and New Worlds, the separation of Greenland from Europe, and of Canada and Newfoundland from Spain. It also witnessed the rise of new islands in the Atlantic.

(7) The seventh period is that of the reign of man, and his endeavour to control and shape the face of the Earth. Buffon, however, held out no great hopes for the future of mankind upon this globe. He believed that the Earth would continue to cool until it would be too cold for any living thing to exist on it.

Buffon made an attempt to estimate the length of the several epochs in the history of the Earth. His estimates were approximately 3,000 years for the first epoch; 32,000 years for the second; 25,000 years for the third; 10,000 years for the fourth; 5,000 years each for the fifth and the sixth epoch; and 5,000 years for the beginning of the seventh epoch, with a future duration of another 93,000 years, at the end of which all life on the Earth would be extinct. These highly speculative and unwarranted estimates are of interest for two reasons, namely, (1) because they mark a definite departure from the few thousand years that were then still counted from the Creation, and, even more so, (2) because Buffon tried to base his estimate of the length of the earlier epochs on experiments with globes of cast iron, and was thus the first to introduce experimentation into the study of geology.

B. PALAEONTOLOGY

The interest shown by the seventeenth century in so-called "figured stones" continued, and indeed increased, in the eighteenth century. For a time there was still a marked tendency to regard them as sports of Nature rather than as fossil remains of once living organisms. The ingenious, if not convincing, compromise suggested by Edward Lhuyd, namely, that some of the "figured stones" contained the remains of marine organisms which had germinated among the

Buffon

JOHANNES JACOBUS
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SCHEUCHZERUS
Soc Reg Londin Acad Sci...
...A. ...Zur...

Scheuchzer
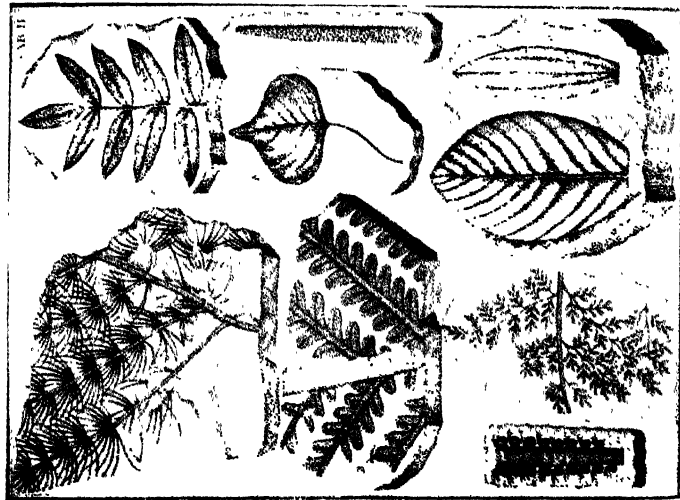
Illustr. 171

Scheuchzer's Illustrations of Fossils (1)

Illustr. 172
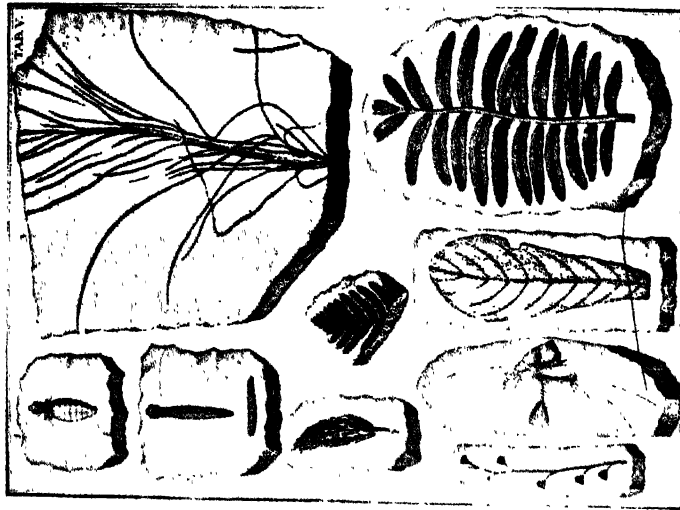
Scheuchzer's Illustrations of Fossils '(2)

rocks from germs accidently carried there in the form of vapour by the rain and wind from the sea was supported by the Swiss geologist Karl Nikolaus Lang in his *Historia Lapidum Figuratorum Helvetiae* (Venice, 1708).

LEIBNIZ

One of the best defences of the fossil character of "figured stones" was made by Leibniz in his *Protogoea*, in which he ridiculed the contention that they could be explained as sports of Nature. To attribute to Nature such gamesome tendencies was merely an attempt to conceal one's ignorance. The objection that some of the alleged fossil remains have no living parallels Leibniz counters by two arguments. In the first place, there are still many unexplored regions where such plants and animals might yet be found. In the second place, he contends that it is only natural to suppose that there have been many changes in animal forms during the many changes that the Earth has undergone, and it is for that reason that the different kinds of fossil remains in the several strata of an area give us a clue to its history. The *Protogoea* of Leibniz, however, was not published till 1749, so that its influence was not as great or as timely as it might have been. In the meantime other influences tended in the same direction.

SCHEUCHZER

If Biblical beliefs sometimes hindered the march of truth, they also advanced it at other times. This is illustrated in the case of Johann Scheuchzer (1672-1733), the most prolific Swiss writer on the fossils of Switzerland. In a work published in 1702, he still maintained the view that figured stones were sports of Nature. Subsequently he read Woodward's *Essay towards a Natural History of the Earth*, and was so smitten with the idea that fossils could be treated as evidence of the Deluge that he not only translated the *Essay* into Latin, but made all his own publications "witnesses of the Deluge." In this way the belief in the Deluge, and an eagerness to maintain its historicity, helped Scheuchzer (and others) to a correct conception of "figured stones." In his eagerness to find a fossil man to bear witness to the Deluge, Scheuchzer mistook a fossil salamander for a human fossil, and made it do duty—*Homo Diluvii Testis* (1726). Yet Scheuchzer was not without a sense of humour. In his *Piscium Querelae et Vindiciae* (1708) fossil fishes are described as holding a meeting of protest against the malicious libels of the descendants of the wicked men who had brought on the Flood, and thereby entombed these very fishes. The libels consist in describing

the fossil fishes as mere sports of Nature. The fossil fishes point to their minute anatomical structure as evidence that they could not have been produced in a mechanical way, but must be accepted as the remains of genuine fishes. Though a dumb race, they claim to bear eloquent witness to the universal Deluge for the benefit of unbelievers. Scheuchzer's most important work is his *Herbarium Diluvianum* (1709), in which he describes many fossil plants, etc., and depicts them in a number of good plates.

KNORR AND WALCH

The most complete account, and the most beautiful illustrations, of the fossils known in the eighteenth century we owe to Georg Wolfgang Knorr (1705–61) of Nürnberg, and Johann Ernst Immanuel Walch (1725–78) of Jena. Knorr was an engraver by profession and a naturalist by inclination. He had already executed many beautiful engravings to illustrate some works on botany and conchology when he became an enthusiastic collector and illustrator of fossils, and decided to prepare a complete treatise on the subject with the aid of fossils contained in numerous other collections. The title of the projected work reminds one of Scheuchzer's eagerness to furnish evidence of the great Deluge—*Lapides Diluvii Universalis Testes*. Knorr, however, only lived long enough to complete the first volume, though he left a lot of material for the rest of the work. Walch, who was Professor of Philosophy and Poetry at Jena, a keen geologist, and the author of a book on rocks (*Das Steinreich*, 1762), was persuaded to carry on the work of Knorr. The entire treatise filled four folio volumes illustrated by approximately three hundred plates. It bore the above-mentioned title in Latin, and a sub-title in German, namely, "A Collection of Natural Curiosities in Proof of a Universal Flood." The publication of the fourth volume, marking the successful completion of a great enterprise, took place in 1778. Notwithstanding its tendentiousness in defence of the diluvial creed, perhaps largely because of it, the work contains a masterly and exhaustive account of all the palaeontological knowledge that had been acquired up to that time.

BERINGER

The correct conception of "figured stones" was certainly gaining ground during the eighteenth century, partly with the help of the diluvial creed. But rival views were not yet dead; and it needed a stroke of humour to aid the work of reason. This was most effectively accomplished in the case of Johannes Bartholomeus Beringer, a supporter of the non-fossil interpretation of "figured stones." He

was Professor in the University of Würzburg, and an enthusiastic collector of "figured stones," which he assiduously gathered from the surrounding country and elsewhere with the help of his students. Amused perhaps by the Professor's over-anxiety to find "figures" in stones, some of the young wits artfully prepared "figured stones," and placed them where he would discover them. Figures of stellar bodies, of letters of the Hebrew alphabet, etc., were all taken seriously by the Professor, who described them, together with genuine "figured stones," in his *Lithographia Würceburgensis* (1726). Encouraged by his gullibility, some of the students went so far as to prepare a "figured stone" with his own name on it, and, as usual, led him where he would discover it. This find at last opened his eyes to the fact that he had been the victim of a long-sustained hoax. He bought up and destroyed as many copies as possible of his *Lithographia*; but the book was reprinted, in 1767, as a curiosity, and it may certainly serve as a warning to uncritical men of science.

C. Volcanic Geology

GUETTARD

Jean Étienne Guettard (1715–86) was born at Étampes, near Paris. In his early years he studied botany and came into contact with the brothers Jussieu of the Jardin des Plantes. Later he became a doctor and was employed by the Duke of Orleans, not only as medical adviser, but also as curator of his natural history collection. Provided with a small pension on the Duke's death, Guettard devoted himself entirely to his favourite studies of botany and geology. He had noticed the association of certain plants with certain minerals and rocks, and was thus led to the study of the distribution of minerals and rocks and of the forces which effected changes in land-surface. In 1746, Guettard communicated to the Paris Academy of Sciences a *Mémoire et Carte Minéralogique*, which may be described as the first attempt at a geological survey. In it he described the arrangement of rocks and minerals in central and northern France, in the light of the observations which he had made in those parts. He suggested that the minerals and rocks were arranged in certain "bands," of which Paris was the centre. In the middle was a more or less oval-shaped sandy band composed of sandstones, millstones, limestones, hard stones and flints. Round it lay a marly band, composed of hardened marls and a few fossils. Round this lay a schistose band, containing the various metals, also bitumen, slate, sulphur, granite, marble, coal and other fossils. All these data were indicated on a map of France, on which it was shown that the three bands were interrupted by the English Channel and the Straits of

Dover. Guettard surmised that the three bands were continued under the sea and on the English shore. He found some confirmation of his conjecture in Joshua Childrey's *Britannia Baconica* (1660) and Gerard Boate's *Ireland's Naturall Historie* (1652), which he read in French versions; and so he continued his survey on a map of England, though less successfully than on the map of his own country. The Academy rightly welcomed this *Mémoire* as the pioneer of "a new field for geographers and naturalists," and as forging a new link between them. Guettard continued his work on a geological survey of France and prepared sixteen maps, which however had to be completed by Monnet, and were published in their joint names (*Atlas et Description Minéralogiques de la France*, 1780). Guettard was also an industrious palaeontologist. He did much to help to establish the real status of "figured stones." He was also the first to identify the trilobite fossils in the slates of Angers; and his name has been given to one of the classes of chalk sponges, *Guettardia*. In the realm of physiographic geology, which likewise received his attention, he emphasized the agency of water in causing land-disintegration, and indeed the agency of subterranean, as well as of surface and rain, water. Guettard's fame, however, rests chiefly on his identification of sixteen or seventeen extinct volcanoes in the centre of France, which he discussed in a "Memoir on certain Mountains in France which were once Volcanoes," read before the Academy of Sciences in 1752 and published in 1756. While travelling about in order to collect materials for his geological survey, his curiosity was aroused by the mile-posts at Moulins on the Allier. They consisted of black stones, and appeared to him to be volcanic in origin. When he was told that they were obtained from Volvic, the very name of their place of origin increased his suspicion, for *Volvic* looked like an abbreviation for *Volcani vicus* (Vulcan's, or volcanic, village). He hastened to the quarries, and found that the rock looked like a solidified stream of lava, which had flowed into the plain to a distance of about five miles. Moreover, the cone and crater were easily recognizable. He then went southward past the Puys to Clermont, whence he ascended the Puy de Dôme (famous as the scene of Pascal's barometric experiments). All round him he saw the cones and craters of extinct volcanoes, and everywhere he found masses of pumice to confirm his view of the volcanic character of the locality; and the presence of hot springs at the foot of some of these mountains clinched the matter for him. Curiously enough, when later on he made a special study of basalt, he failed to recognize its volcanic character, although he had noticed its presence in volcanic areas. Misled by the accident that he had never seen basalt in columnar form, he concluded that it was "a species of vitrifiable rock formed by crystalliza-

tion in an aqueous fluid" (*Memoir on the Basalt of the Ancients and Moderns*, 1770). By a strange fate Guettard thus became the father of two rival schools of geology, namely, the Vulcanists (or Plutonists) and the Neptunists, of whom more will be said presently. His discoveries in connection with the old volcanoes in Auvergne inspired the Vulcanists; his view of the aqueous origin of basalt provided an important plank in the platform of the Neptunists.

DESMAREST

The work of Guettard was continued, if not always fully appreciated, by his younger compatriot, Nicholas Desmarest (1725–1815), a native of Soulaines. He was brought up in such straitened circumstances that he could hardly read when he was fifteen; and it was only thanks to the insight of some teachers that he obtained a free education, first at the College of the Oratorians at Troyes, and finally in Paris. Even afterwards he never knew anything but laborious days, and his whole life stands out as an example of plain living and high thinking. In 1752 he won a prize for an essay on a subject suggested by Buffon's *Theory of the Earth*, namely, whether England and France had ever been connected by land. He answered the question in the affirmative, basing his conclusion partly on the evidence suggested by Guettard, namely, the continuity of the same geological bands in the two countries and on the bottom of the straits between them, and partly on the former presence in England of wild animals which could only have got there from the Continent at a time when England and France were still joined by a strip of land which has since then been washed away by the North Sea. This essay procured for Desmarest some distinguished patronage, and in 1757 he was appointed to a small government post, which in 1788 developed into that of Inspector-General and Director of the Manufactures of France. In this capacity he did much to advance the economic and industrial progress of his country, but the extensive journeys which it necessitated gave him ample opportunities for fruitful geological observations. For a short time during the Revolution he was kept in prison. But his habit of making his journeys on foot, living on bread and cheese, and sleeping in shepherds' huts, did not suggest aristocratic leanings. Besides, he was a valuable economic asset. So he escaped with his life, and was eventually restored to his office, which he carried on till the end.

It was in the volcanic region of Auvergne that Desmarest, like Guettard, had his attention directed to the study of basalt. During his first visit there, in 1763, Desmarest, more fortunate in this respect than Guettard had been, observed some columnar basalt rocks in

the vicinity of the old volcanoes, indeed all along the edge of the lava. The circumstances in which he found them convinced him that prismatic basalt was a volcanic product, and that its regular shape was the result of the fusion of the underlying granite by volcanic fire. The basalt rocks of the Giant's Causeway in North Ireland were among the most widely discussed wonders of those days, and Desmarest had read a great deal about them, also about somewhat similar columns in various parts of Germany. The scenery round the Giant's Causeway, so far as he could judge from pictures, looked very like that of parts of Auvergne; and the colour hardness, and texture of the basalt columns in both localities appeared to be just the same. He concluded that the North Irish coast, and indeed all places in which such basalt rocks are found, must have been the scene of extinct volcanoes. After years of continued study of these problems he realized that volcanic action had been even more extensive than Guettard had suspected, and that the Continent showed two vast regions of old volcanic activity, namely, (1) an eastern region extending from the borders of Saxony and Bohemia to Silesia, from Freiberg to Lignitz, and (2) a southern region extending from near Cologne to Nassau, Hesse, Darmstadt, and Cassel. Desmarest first published some of his conclusions in the *Memoirs of the Academy of Sciences* in 1774 and 1777, though he had addressed the Academy on the subject in 1765 and again in 1771. The most important parts of these *Memoirs* are those dealing with the main types of lava-deposits, and their relations to one another. He returned to the same theme in 1775 in a paper which he read before the Academy of Sciences "On the Determination of Three Epochs of Nature from the Products of Volcanoes, and on the Use that may be made of these Epochs in the Study of Volcanoes" (published in abridged form in *Journal de Physique*, 1779; completely, in *Mém. de l'Instit. des Sciences Math. et Phys.*, 1806). The first (i.e., the most recent) epoch embraces the latest lava-deposits of volcanoes that are either still active or have become extinct in comparatively recent times. They show crater-bearing cones and sheets of rugged, dark, verdureless lava extending from the craters into the surrounding region. In some cases the cones show signs of wear, the scoriae have been shifted to the lower levels, and the lavas are partly trenched. These changes have been wrought by rain or melted snow. In some cases the flowing water has cut the sheet of lava, and formed a valley across it. The second (earlier) epoch embraces the lava-deposits whose crater-bearing cones, scoriae and slags have been washed away, and which have been broken up into patches of table-land by the valleys cut through them by the running water. The third, or most ancient, epoch embraces the lava-deposits which lie

under the sedimentary strata or are interstratified with them. This epoch must have extended over a long period to allow of the deposition of from 600 to 900 feet of horizontal sediments above the oldest lavas. Desmarest regarded volcanic eruptions as mere incidents in the continuous operations of Nature through the agencies of weather and water. The heated controversy between Neptunists and Vulcanists, which was in full swing then, might have been avoided if the rival protagonists had studied the work of Desmarest, who ignored the whole dispute.

Mention must also be made here of Desmarest's famous volcanic maps and his work on *Physical Geography* (4 vols., 1794–1811).

DE SAUSSURE AND PALLAS

Desmarest's view that basalt was formed by the fusion of granite by volcanic fire was put to an experimental test by Horace Bénédict de Saussure (1740–99), of Geneva. He fused a number of different granites, Swiss and French, but he failed to reduce any of them to basalt. He also experimented with combinations of granite with schorl and with various porphyries, but he failed to get basalt by their fusion. He therefore concluded (incorrectly, as will appear presently) that basalt was not produced by fusion, as Desmarest had thought. Although these experiments were negative in their result, they give De Saussure a claim to be regarded as one of the earliest experimenters in the domain of geology. But this was not his sole title to fame. It was he who first gave vogue to the terms "geology" and "geologist." And, above all, his *Voyages dans les Alpes* (3 vols., 1779, 1786, 1796) not only gave a great stimulus to mountaineering and to geological field-work, but also provided a vast store of reliable geological information, which was turned to good account by other and more original geologists, like Hutton and others. For similar reasons mention may be made here of the work of Peter Simon Pallas (1741–1811), a native of Berlin, who did for the geology of Russia what De Saussure did for the geology of Switzerland, and did it under peculiarly difficult conditions. His researches are described in his *Consideration of the Structure of Mountain-Chains* (1771) and in his *Physical and Topographical Sketches of Taurida* (1794), both of which were published by the Academy of St. Petersburg.

MICHELL

From the study of volcanoes we may turn for a brief while to the study of earthquakes. Considerable alarm was caused throughout Europe by a number of earthquakes which shook various West-European countries in 1750; and the alarm became a panic when

the catastrophic Lisbon earthquake followed in 1755. Naturally the subject of earthquakes attracted the attention of the learned world, and the publications of scientific societies contained a number of attempts at the elucidation of these phenomena. The most valuable of these contributions was that made by John Michell (1724–93) in an "Essay on the Causes and Phenomena of Earthquakes," which he communicated to the Royal Society in 1760 (*Phil. Trans.*, Vol. XLIX). In it he pointed out that earthquakes commonly occur in the vicinity of volcanoes and at the time of their eruption. The hypothesis which he maintains is that earthquakes are the effect of a sudden contact of subterranean fires with large masses of water, which are consequently evaporated, and cause a shock by their elastic force. When subterranean fires find a vent through a volcanic crater, even then the disturbance produced is extensive; but when these fires find no such escape, and the roof above them falls in, then the disturbance is naturally far more extensive. For the water contained in the cavities of the roof falls into the fire, is immediately evaporated, thereby creating a cavity between the molten matter and the rock above it, which by its alternate compression and expansion produces a vibration at the surface. In this way waves are propagated through the crust of the Earth, the amplitude of these waves being greatest immediately above the source of the disturbances, and diminishing gradually with their distance from it until they die away. The idea of these earth-waves was original, and also suggested to the author a method of locating the focus of origin of an earthquake. If lines are drawn through a number of the observed paths of the earth-waves, then the point of their intersection should be near the required focus. By this method he tried to compute the focus of origin of the Lisbon earthquake, and located it under the Atlantic between the latitudes of Lisbon and Oporto, at a depth of from one to three miles. With all its faults, Michell's *Essay* must be regarded as the beginning of scientific seismology.

D. PHYSICAL GEOLOGY

The problem of the nature of the principal types of strata in the crust of the Earth, their origin and temporal sequence, had received some attention in the seventeenth century, notably in Steno's little book (*De solido intra solidum naturaliter contento*, 1669; Eng. trans. by H. O., as *Prodromus*, etc., 1671). The subject attracted much more attention during the eighteenth century, largely in consequence of the general interest aroused by Buffon's speculations. Anyway, this group of problems constituted the major issue of eighteenth-century

geology; and considerable progress was made towards its solution in spite of the obstruction caused by the futile controversy between Neptunists and Vulcanists (or Plutonists). The work was carried on to a considerable extent by investigators who were familiar with mines, more particularly with British coal-mines. Praiseworthy attempts at a classification of strata were made, early in the century, in England, Italy, and Germany.

STRACHEY

John Strachey (1671–1743) studied the various types of geological formations, and their sequence, in the south-west of England. He published the results of his investigations in two papers giving "A curious Description of the Strata observed in the Coal-mines of Mendip in Somersetshire" (*Phil. Trans.*, 1719), and "An Account of the Strata of Coal-mines," etc. (*Phil. Trans.*, 1725), and in a book entitled *Observations on the Different Strata of Earths and Minerals*, etc. (1727.) In these writings Strachey described in correct order the principal divisions of the stratigraphical series from coal to chalk, and drew attention to the fact that the coal-strata are inclined, whereas the overlying strata, from red marl upwards, are horizontal, lying across the edges of the coal-strata.

ARDUINO

Giovanni Arduino (1713–95), a native of Verona and Professor in Venice, made a special study of the rocks of North Italy. He gave an account of his conclusions in "Two Letters" which he addressed to Antonio Vallisneri in 1759, and which were published in A. Calogiera's *Nuova Raccolta d'Opuscoli scientifici*, etc., in 1760. These "Letters" are noteworthy because they contain the first classification of rocks into *primitive, secondary, tertiary*, and *volcanic*. In the primitive class Arduino included the schistose rocks, which form the core of mountains, and contain no fossils. In the secondary class he included limestones and marls, clays and shales, and other stratified rocks which contain numerous marine fossils. The tertiary class includes more recent limestones and marls, clays and sands, etc., which have been formed from materials derived from disintegrated secondary strata, and contain only terrestrial fossils. The volcanic rocks form a separate class or sub-class, consisting of lavas and tuffs caused by eruptions and inundations.

LEHMANN

A somewhat similar classification of strata was advocated about the same time by Johann Gottlob Lehmann (*d.* 1767), Professor in

Berlin and later in St. Petersburg. He made a close study of the rocks in the Harz Mountains and in the Erzgebirge, and published an account of them in his *Essay on the History of Flötzgebirge*, or stratified rocks (Berlin, 1756). The first of his three classes contains the oldest rocks or mountains, which reach down to unknown depths and rise to the greatest heights, show little variety, and are vertical or inclined, never horizontal. The second class contains the stratified rocks which have been formed from the sedimentary deposits of water and whose strata are horizontal and lie in a regular order, the coarsest sediments being at the bottom and the limestone coming on the top. The third class contains still later strata which have resulted from local accidents at various periods subsequent to the formation of rocks of the first and second cl

FUCHSEL

Perhaps the most important of this group of eighteenth-century pioneers whom we are now considering was Lehmann's contemporary and compatriot, Georg Christian Füchsel (1722–73), a native of Ilmenau in Thüringen, and physician to the Prince of Rudolstadt. While still a student he discovered a coal seam at Mühlberg near Erfurt, and subsequently interested himself in the geology of Thüringen. In 1762 he published a long Latin essay giving "A History of the Earth and the Sea, based on a History of the Mountains of Thüringen" (*Trans. Elect. Soc. Mayence*, Vol. II), and in 1773 he published, in German, *A Sketch of the Oldest History of the Earth and Man.* The earlier essay contained a detailed geological map, the first of its kind, of Thuringen; it also defined carefully a number of geological terms, such as *stratum, formation (series montana)*, etc. By *formation* he understood a number of strata formed in immediate succession and in sufficiently similar circumstances to represent one geological epoch. He identified nine such formations in Thüringen:

(1) The oldest consists of the vein series of vertical rocks which form the summits of the Thüringen and Harz Mountains.

(2) The second, in order of age, is the carboniferous series.

(3) The third series consists of slates with layers of marble.

(4) The fourth series consists of red rocks, with intercalations of red marble.

(5) The fifth formation consists of white rocks, with layers of clay and sand.

(6) The sixth formation consists of the metalliferous or Permian series and copper slate.

(7) The seventh series consists of granular limestone and dolomite marls or Zechstein dolomite.

(8) The eighth formation consists of the sandstone series, or Bunter sandstones.

(9) The ninth and most recent formation consists of the Muschelkalk or upper limestone series. Füchsel, moreover, noted carefully the kinds of fossils found in the several formations, such as land plants with coal, gryphites with Zechstein, and ammonites with Muschelkalk. He also noted that some formations contain only terrestrial fossils, indicating ancient land, whereas others contain only marine fossils, indicating the former presence there of the sea.

The fundamentals of a scientific stratigraphy had thus been brought to light by these pioneers. But the foundations which they laid were long neglected, as, for one reason or another, their writings received little or no attention till long afterwards. In the meantime a more loud-voiced school of geology made its appearance, dominated the geological world during the rest of the eighteenth century, and hindered its scientific progress to no small extent, while doing much to make geological studies popular and fashionable. The school in question was the Wernerian school of "geognosy," or the Neptunist school.

WERNER

Abraham Gottlob Werner (1749–1817) was a native of Wehrau, in Saxony, where for a time he was in charge of a smelting-house in an iron foundry, but left in order to study at the School of Mines in Freiberg, and then at the University of Leipzig. In 1774 he published a little book in German on *The External Characters of Minerals*, which made a very favourable impression by the unusually methodical way in which the subject was dealt with. In 1775 he was appointed Curator of Collections and Teacher of Mining at his old school in Freiberg. He held this post for the rest of his life, and made his school the most famous school of geology in the world, attracting numerous students from all countries. His great fame did not rest on his writings, which were indeed very few, comprising only a pamphlet called *A Brief Classification and Description of the Different Kinds of Rocks* (Dresden, 1787), a number of mineralogical papers, and a little book entitled *A New Theory of the Origin of Mineral Veins* (1791; translated by Charles Anderson, 1809), in addition to the above-mentioned treatise on minerals. But he was an exceptionally attractive lecturer whose very dogmatism helped to fill his students with enthusiasm for his views, so that they went forth as disciples and missionaries of his geological creed. A full account of Werner's views is only obtainable from the writings of his devoted disciples, notably Franz Ambros Reuss, D'Aubisson de Voisins, and Robert

Jameson, Professor in the University of Edinburgh and author of *Elements of Geognosy* (Edinburgh, 1808). He won the interests of his audience not only by the simplicity and orderliness of his treatment of the subject, but also by his excursions into wider realms of human interest. Though he began his lectures with minerals, he did not end there, but would expatiate on the influence of the distribution of minerals upon the migrations and characters of peoples, upon the arts and crafts of human life, upon history, politics and warfare, in short, upon the whole destiny of civilization. Certainly nobody could do more to make the study of geology popular.

In spite of his addiction to these speculative flights in his lectures, Werner professed a contempt for the speculative geologists who devoted themselves to "geogony" (that is, to theories about the origin of the Earth), and, for that reason, preferred to describe his subject as "geognosy," which he defined as "the science which inquires into the constitution of the Earth, the arrangement of minerals in the various layers of rock, and the mutual correlation of the minerals." He prided himself on his respect for observed facts, and avoidance of all speculative theory. Yet he had no compunction in making generalizations about the whole Earth from his very limited experiences in Saxony; and he turned all his hypotheses into "facts," or certainties, by the simple process of asserting them with emphasis, as will appear presently from a quotation in which he summarized his views.

Like several of his predecessors, whose views have already been dealt with, Werner believed in the regular recurrence of certain series, "suites," or formations of rocks, each such formation being characteristic of a certain epoch in the history of the Earth. Guided by his observations in Saxony, he enunciated the existence of five such formations, which have come into being in the following order of time:

(1) First came the *primitive rocks*, devoid of all fossils. They comprise granite, gneiss, mica-slate, chlorite schist, primitive greenstone and limestone, quartzite, serpentine, porphyry, syenite, etc.

(2) Next in order of age came the *transitional rocks*, containing some fossils. They include the mica-slate series, crystalline schist, greywacke, transitional greenstone, and gypsum.

(3) The third group comprises the sedimentary or *floetz rocks*, including sandstone, coal, limestone, the metalliferous rocks, bituminous lignite, muschelkalk, freestone and chalk, basalt, brown coal, obsidian, rock-salt, etc.

(4) The fourth class consists of the *transported or derivative rocks*, including sand, clay, pebbles, calcareous tufa, bituminous wood, soapstone, and aluminous earth.

(5) The fifth, and the most recent, group comprises the *volcanic rocks*, including both the genuinely volcanic (namely, lava, volcanic scoriae and ashes, pepperino, and tuff) and the pseudo-volcanic (namely, burnt clay, jasper, polishing stone, and slag).

In Werner's explanation of the mode of origination of these various formations the sea plays the supreme role. Hence the name of Neptunism for this school of geology. According to Werner, the Earth consisted originally of a solid nucleus completely enveloped in an ocean of water, which was at least as deep as the mountains are high. The primitive rocks were formed by the chemical crystallization of the rock-material which the great ocean held in solution. Of the transitional rocks some (namely, the slates and shales) were formed by chemical precipitation, and the rest (the greywackes, for instance) were laid down by mechanical sedimentation. The floetz rocks originated during alternating periods of calm and of disturbance, when the waters sometimes, by receding, gave rise to new continents, and at other times, by inundation, submerged existing land-areas. Similar conditions accompanied the formation of the transported or derivative rocks. The volcanic rocks appeared last of all, according to Werner. As he did not credit the Earth with any kind of internal fire or other internal reservoir of energy, volcanic rocks could only be regarded by him as recent, accidental products, to be accounted for by the old view that they resulted from the combustion of the coal which had accumulated in the crust of the Earth, and that they had come into existence long after the four main formations had been laid down by the sea. And, in spite of Desmarest's painstaking evidence to the contrary, Werner advocated the view that basalt is not volcanic but of aqueous origin, as Guettard had maintained.

Werner's classification of rocks was delightfully methodical and simple. Anybody who had inspected a Saxon mine under his guidance knew exactly what to expect in any other part of the Earth's crust. And the genetic explanations were even more simple. All the fundamental types of rock were merely either the chemical precipitates or the mechanical sediments of the great ocean. Such physico-chemical questions as to whether water could hold granite, metals, etc., in solution, were either not raised or glossed over. And the essential doctrines were taught in a spirit of such transparent confidence that it seemed irreverent to challenge them, even if some of the phrases used did not seem to make good sense. The following utterance of Werner may be of interest both as a summary of his theory and as an example of his supreme self-confidence.

"In recapitulating the state of our present knowledge it is obvious that we know with certainty that the floetz and primitive mountains

have been produced by a series of precipitations and depositions formed in succession from water which covered the globe. We are also certain that the minerals which constitute the beds and strata of mountains were dissolved in this universal water and were precipitated from it; consequently the metals and minerals found in primitive rocks, and in the beds of floetz mountains, were also contained in this universal solvent, and were formed from it by precipitation. We are still further certain that at different periods, different minerals have been formed from it, at one time earthy, at another time metallic minerals, at a third time some other minerals. We know, too, from the position of these minerals, one above another, how to determine with the utmost precision which are the oldest, and which the newest precipitates. We are also convinced that the solid mass of our globe has been produced by a series of precipitations formed in succession (in the humid way); that the pressure of the materials, thus accumulated, was not the same throughout the whole; and that this difference of pressure and several other concurring causes have produced rents in the substance of the Earth, chiefly in the most elevated parts of its surface. We are also persuaded that the precipitates from the universal water must have entered into the open fissures which the water covered. We know, moreover, for certain that veins bear all the marks of fissures formed at different times; and, by the causes which have been assigned for their formation, that the mass of veins is absolutely of the same nature as the beds and strata of mountains, and that the nature of the masses differs only according to the locality of the cavity where they occur. In fact, the solution contained in its great reservoir (that cavity which held the universal water) was necessarily subjected to a variety of motion, whilst that part of it which was confined to the fissures was undisturbed, and deposited in a state of tranquility its precipitate" (*Theory of Mineral Veins*, Eng. trans., p. 110).

While Werner and his disciples were carrying the flag of Neptunism triumphantly, some careful geological work was being carried on unobtrusively in the British Isles which was destined to undo the mischief done by the Freiberg school, and to lead geology back to a more scientific path. The head of this counter-movement was Hutton, whose vindication of the reality of subterranean sources of heat, and their share in shaping some of the formations of the Earth's crust, led to the application of the name Vulcanism or Plutonism to this school of geology, in contrast with the Neptunism of Werner's school of thought.

James Hutton (1726–97) was born in Edinburgh, and attended the local High School. At seventeen he was apprenticed to a lawyer, but left soon in order to study medicine, first at the University of his native city, then in Paris, and finally in Leiden, where he graduated as Doctor of Medicine in 1749. But he never practised medicine. For a time he carried out various chemical experiments, including some with sal ammoniac, from the manufacture of which he eventually derived a sufficient income to enable him to pursue those unremunerative geological studies on which his fame rests. In 1752 he went to a farm in Norfolk to study agriculture, and from 1754 till 1768 he managed his own farm in Berwickshire; but he then let it, and settled in Edinburgh. Here he associated with Joseph Black, John Playfair (Professor of Mathematics), and Sir James Hall, among others, and devoted himself entirely to his studies. These were by no means confined to geology. He published treatises on physics and metaphysics some time before his classical *Theory of the Earth* appeared. Although he had been interested in geology a great many years, had made geological tours in Scotland, England, Wales, and on the Continent, and had actually conceived his theory of the Earth and communicated it orally to his friends, he did not write on the subject until he was persuaded to address the then newly constituted Royal Society of Edinburgh, in 1785. In that year he gave them two papers, which were published in the first volume of the society's *Transactions* under the title "Theory of the Earth; or an Investigation of the Laws observable in the Composition, Dissolution, and Restoration of Land upon the Globe." The theory seems to have attracted little attention. But in 1793 it was attacked in a rather offensive manner by Richard Kirwan (1733–1812), subsequently President of the Royal Irish Academy. Thereupon Hutton set to work to elaborate his theory in greater detail, and to furnish fuller evidence in support of it. The result was the *Theory of the Earth, with Proofs and Illustrations*, in two volumes, published in 1795. Six chapters of a projected third volume were published, by the Geological Society of London, in 1899.

Hutton had pondered problems of scientific method, and had published a voluminous work on the problems of knowledge (*An Investigation of the Principles of Knowledge, and of the Progress of Reason from Sense to Science and Philosophy*, 3 vols., 4°, 1794). The methods he pursued were therefore not haphazard or unconscious, but deliberately chosen. They may be described briefly as the methods of naturalism. The past history of the Earth must be explained by reference to such natural operations as are still observable, or have been observed comparatively recently, and without having resort

to the agency of any preternatural cause. In science, at all events, natural phenomena must be conceived as forming a self-contained system unaffected by supernatural incursions (*Theory of the Earth*, Vol. II, p. 547).

Like others before him, Hutton had observed that the rocks below the covering of soil usually consisted of parallel strata arranged in a definite order, and that although the several strata were different in composition, they all appeared to consist of the detritus of older rocks. Moreover, exactly similar strata could be observed in process of formation under the sea. It seemed, therefore, that most of the land, as we know it now, had probably been formed from the detritus of previously existing land, which had spread horizontally as a sediment over the bed of the sea, where it had become compacted into solid stone. It was consequently reasonable to suppose that the Earth was at one time enveloped in an ocean of water, in the bed of which the detritus of its nucleus of primitive rocks was compacted into solid strata. But Hutton did not believe that pressure alone could account for the consolidation of soft sediments into solid stones. He maintained that it required the action of subterranean heat, though he also realized that the ordinary effects of heat on rocks were modified by their subjection to pressure. His view seemed to be confirmed by the fact, to which Steno had already drawn attention, that land strata often deviate from the horizontal position which they must have had as sea-sediments, and are found in all sorts of irregular positions—inclined, folded, crumpled, ruptured, and with truncated ends in a vertical position. Occasionally also, as he pointed out, one finds in close juxtaposition strata which are usually far apart and belong to quite different formations. Such things, according to Hutton, can only be explained by reference to convulsions caused by reservoirs of heat in the Earth's interior, and modified by the resistance of the masses on which it acted, so as to produce all kinds of contortions and irregularities in the crust of the Earth. Volcanoes, according to Hutton, are but spiracles or safety-valves through which the Earth's internal heat sometimes escapes, and so "prevent the unnecessary elevation of land, and fatal effects of earthquakes" (*Theory*, Vol. I, p. 146).

In this way Hutton arrived at the notion that the Earth's interior might contain "a fluid mass, melted, but unchanged by the action of heat"; and he proceeded to explain various other geological phenomena by supposing some of this fluid molten material to have been forced from the interior to the crust of the Earth, where it intruded into various strata from below. Whinstones (including the much-disputed basalts), porphyry, and granite were conceived by Hutton as such intrusive rocks. Veins of granite and whinstone were

accordingly regarded by him as the result of the outflow of igneous material from below, and not as formed by precipitation from an overlying sea.

Basalt was definitely conceived by Hutton as volcanic in origin, and he first explained the difference between basalt (and similar rocks) on the one hand, and the ordinary streams of lava on the other. Basalt has a crystalline structure because the lava of which it was composed did not reach the surface of the Earth but remained subterranean, and solidified under the enormous pressure of over-lying rocks. The lava which escapes at the surface of the Earth has not been subjected to such pressure, and is consequently vesicular in structure. Hutton's hypothesis was subsequently verified experi-mentally by Sir James Hall.

Although Hutton was a Plutonist, it should be clear now that he did not deny the vast influence exercised by water, as well as by heat, in the drama of geological transformation. On the contrary, he has given us some of the most impressive descriptions of the way in which the face of the Earth is being incessantly changed by the action of water, as well as by the action of air and heat. Atmos-pheric weathering and chemical disintegration decompose the rocks; rain and running water wash the detritus into the sea or other water reservoirs, where it accumulates and forms new strata to be heaved up in due course by some internal convulsion. In this way old lands are washed away, and new ones arise; and the trans-formation continues slowly but incessantly, suggesting "no vestige of a beginning, no prospect of an end."

Particularly interesting, for physical geography, was Hutton's idea of earth-sculpture by running water. The idea was most beautifully expressed in Playfair's *Illustrations of the Huttonian Theory*, from which the relevant passage is quoted on pages 424 f.

PLAYFAIR

Hutton was rather lacking in the power of literary expression, and the spread of his ideas might consequently have been retarded much more than it was but for the help of his friend Playfair, whose *Illustrations of Huttonian Theory* (1802) rendered the utmost service in making Hutton's thought more intelligible and more interesting. Playfair, however, also contributed ideas of his own. The most original of these was his recognition of the geological function of glaciers. "For the removal of large masses of rock," he pointed out, "the most powerful engines without doubt which nature employs are the glaciers, those lakes or rivers of ice which are formed in the highest valleys of the Alps and other mountains of the first order"

(*Illustrations*, p. 388). Geologists and others had long puzzled over the question of the transport of huge erratic boulders to their unexpected places on the slopes and in the plains of the Alps; but no plausible explanation was forthcoming until Playfair thought of the potentialities of glaciers past and present. He also furnished the fullest evidence of oscillations in the level of European lands, and showed that certain beaches on the coast of Scotland must have been raised by a rise in the level of the land, and not by a mere fall in the level of the sea.

HALL

Even more important for the progress of geology and the triumph of Plutonism over Neptunism was the work of Hutton's other friend, Sir James Hall (1762–1831). Hall may rightly be described as the father of experimental geology. It is true that some such experiments had been made before by others. Mention has already been made of the experiments of Buffon and De Saussure. Hall's experiments, however, were much more extensive and systematic, and were conducted with a much deeper insight. Hutton himself did not attach much importance to experiment, for he felt a distrust in "superficial reasoning men" who "judge of the great operations of the mineral kingdom from having kindled a fire and looked into the bottom of a little crucible" (*Theory*, Vol. I, p. 251). When one recalls De Saussure's inference from inadequate experiments that basalt could not have been formed by fusion, Hutton's distrust does not seem unreasonable. But Hall was a man of different calibre. He had learned from Black and Hutton the potential importance of such things as variations of pressure when things are being heated, or variations in the rate of cooling when they are allowed to cool down. Experiments guided by such ideas were naturally more valuable and conclusive than the earlier sporadic and rather crude experiments could have been.

Hall's principal experiments had the effect of disproving the main objections which were raised by the Wernerian Neptunists against Hutton's Plutonism. The objections were chiefly these.

(1) In the first place, it was argued against Hutton's Plutonism that crystalline rocks like granite and whinstone could not have been formed out of molten rock-magma, but only by precipitation from the sea, because if such molten matter cooled, it would become glassy and amorphous, not crystalline.

(2) In the second place, it was argued that limestone could not have been formed out of similar matter in a state of fusion, because when so heated all the carbonic acid would have been set free and escaped, and the result would have been quicklime, not limestone.

(1) Hall disproved the first of the above arguments by showing experimentally that whinstones and specimens of lava from Vesuvius and Etna could be fused by heat and could then be cooled into either glassy or crystalline rock according as the molten magma was made to cool rapidly or slowly. For these experiments he used the reverberatory furnace of an iron-foundry. But the experiment was suggested to him by an accident at the Leith glass factory. A quantity of green glass had been allowed to cool slowly and lost its glassy characteristics, becoming opaque and crystalline; but when a part of it was melted again and made to cool quickly, it resumed its glassy quality. In this way he was led to make a number of experiments on the effect of variations in the rate of cooling, verified Hutton's views on the igneous formation of crystalline rocks, and disproved the contention of the Neptunists that crystalline rocks must be aqueous in origin. Incidentally these experiments also explained further the difference between the two kinds of lava which Hutton had noted. The lava which rises in a cold fissure and cools quickly at the open surface becomes vitreous; the inner lava cools slowly, and becomes crystalline.

(2) The second objection was similarly refuted by suitable experiments. He put some powdered limestone into porcelain tubes, gun-barrels, and tubes bored through solid iron, sealed them carefully, and then submitted them to the highest temperatures obtainable at that time. Subjected to the great pressure of the confined and super-heated air, the limestone fused without losing its carbonic acid, and took on the semblance of marble.

Hall carried out hundreds of experiments in support of Hutton's theory. The only other experiment that need be cited here is that by which he showed that sand at the bottom of an iron vessel filled with sea-water could be turned into solid sandstone merely by heating it at a red heat.

Hall's accounts of his experiments are contained in the *Transactions* of the Royal Society of Edinburgh from 1790 onwards (Vols. III–VII, etc.). He was President of the Society for a time.

The eighteenth century had, of course, many other geologists, whose work, however, though important in its way, need not be dealt with in a general history of science. The closing decades of the century also witnessed the preparatory investigations of a number of men who were destined to become famous in the annals of science, but the fruit of their labours only matured and became known in the early decades of the nineteenth century, to which therefore they properly belong.

(See A. Geikie, *Founders of Geology*, 2nd ed., 1905; K. A. von Zittel, *History of Geology and Palaeontology*, 1901; F. D. Adams, *The Birth and Development of the Geological Sciences*, Baltimore and London, 1938; K. F. Mather and S. L. Mason, *A Source Book in Geology*, New York and London, 1939.)

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